THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XV.—*On the Nebular Hypothesis;* by Professor DANIEL KIRKWOOD, Bloomington, Indiana.

THE records of our planet's physical history, from the dawn of organized existence down to the epoch of man's creation, have, for the most part, been brought to light since the commencement of the nineteenth century. Within this brief period the immense antiquity of our globe, the former high temperature of its surface, and, in short, all the main facts and doctrines of geology, have been discovered or confirmed by a laborious examination of the planetary crust. *But the earth has also a pre-geologic history*—a history as yet undeveloped; the grand outlines of which must be derived chiefly from *celestial* phenomena. "The testimony of the rocks" in regard to the one, may be more explicit than that of the *stars* in regard to the other. It must be borne in mind, however, that this department of research—the tracing of astronomical facts not accounted for by gravitation, to their source in the origin of the system—has hitherto received but little attention from men of science. Cogent arguments, it is true, were adduced by Laplace and Pontécoulant in favor of the nebular hypothesis, but very little has since been accomplished, tending either to invalidate or confirm it.

The present article is designed as a popular rather than a scientific discussion of this interesting subject, and we trust its interest will not be abated by the fact that a portion of the matter has been before presented by us anonymously in a Quarterly Review. We shall in the first place present a brief account of the origin and nature of Laplace's theory; secondly, a connected

view of the principal phenomena by which it is sustained; and thirdly, consider the most prominent of the objections which have been urged against it.

As a group, our solar system is comparatively isolated in space; the distance of the nearest fixed star being at least seven thousand times that of Neptune, the most remote known planet. Besides the central or controlling orb, it contains, so far as known at present, sixty-seven primary planets, twenty-one satellites, three planetary rings, and nearly eight hundred comets. In taking the most cursory view of this system of bodies, we cannot fail to notice the following interesting facts in regard to the motions of its various members:—

1. The sun rotates on his axis from west to east.
2. The primary planets all move nearly in the plane of the sun's equator.
3. The orbital motions of all the planets, primary and secondary, except the satellites of Uranus and perhaps those of Neptune, are in the same *direction* with the sun's rotation.
4. The direction of the rotary motions of all the planets, primary and secondary, in so far as has been observed, is identical with that of their orbital revolutions; viz., from west to east.
5. The rings of Saturn revolve about the planet in the same direction.
6. The planetary orbits are all nearly circular.
7. The *cometary*, is distinguished from the *planetary* portion of the system by several striking characteristics: the orbits of comets are very eccentric and inclined to each other, and to the ecliptic at all possible angles. The motions of a large proportion of comets are *from east to west*. The physical constitution of the latter class of bodies is also very different from that of the former; the matter of which comets are composed being so exceedingly attenuated, at least in many instances, that fixed stars have been distinctly visible through what appeared to be the densest portion of their substance.

None of these facts are accounted for by the law of gravitation. The sun's attraction can have no influence whatever in determining either the *direction* of a planet's motion, or the eccentricity of its orbit. In other words, this power would sustain a planetary body moving from east to west, as well as from west to east; in an orbit having any possible degree of inclination to the plane of the sun's equator, no less than in one coincident with it; or, in a very eccentric ellipse, as well as in one differing but little from a circle. The consideration of the coincidences which we have enumerated led LaPlace to conclude that their explanation must be referred to the *mode* of our system's formation—a conclusion which he regarded as strongly confirmed by the contemporary researches of Sir William Herschel. Of the numer-

ous nebulæ discovered and described by that eminent observer, a large proportion could not, even by his powerful telescope, be resolved into stars. In regard to many of these, it was not doubted that glasses of superior power would show them to be extremely remote sidereal clusters. On the other hand, a considerable number were examined which gave no indications of resolvability. These were supposed to be a species of self-luminous, nebulous matter—the chaotic elements of future stars. The great number of these irresolvable nebulæ, scattered over the heavens and apparently indicating the various stages of central condensation, very naturally suggested the idea that the solar system, and perhaps every other system in the universe, originally existed in a similar state. The sun was supposed by Laplace to have been an exceedingly diffused, rotating nebula, of spherical or spheroidal form, extending beyond the orbit of the most distant planet; the planets as yet having no separate existence. This immense sphere of vapor, in consequence of the radiation of heat and the continued action of gravity, became gradually more dense, which condensation was necessarily attended by an increased angular velocity of rotation. At length a point was thus reached where the centrifugal force of the equatorial parts was equal to the central attraction. The condensation of the interior meanwhile continuing, this zone was detached, but necessarily continued to revolve around the central mass with the same velocity that it had at the epoch of its separation. If perfectly uniform throughout its entire circumference, which would be highly improbable, it would continue its motion in an unbroken ring, like that of Saturn; if not, it would probably collect into several masses, having orbits nearly identical. "These masses should assume a spheroidal form, with a rotary motion in the direction of that of their revolution, because their inferior particles have a less real velocity than the superior; they have therefore constituted so many planets in a state of vapor. But if one of them was sufficiently powerful, to unite successively by its attraction, all the others about its centre, the ring of vapors would be changed into one spheroidal mass, circulating about the sun, with a motion of rotation in the same direction with that of revolution."* Such, according to the theory of Laplace, is the history of the formation of the most remote planet of our system. That of every other, both primary and secondary, would be precisely similar.

If it be said that the small eccentricities of the planetary orbits, the approximate coincidence of their planes with that of the solar equator, and the uniformity of direction in which the planets move, are *ultimate facts* that the *final cause* of these arrange-

* Harte's Translation of Laplace's System of the World, vol. ii, note vii.

ments was the stability of the system; and that we transgress the legitimate domain of scientific research in *attempting* their explanation;—we ask by what rule in philosophy the arrangements in question are determined to be ultimate facts. Even granting their final cause to have been the stability of the system, we are by no means to conclude that they are necessarily unsusceptible of explanation. "Final causes," says Whewell,* "are to be excluded *from physical inquiry;* that is, we are not to assume that we know the object of the Creator's design, and put this assumed purpose in the place of a physical cause. We are not to think it a sufficient account of the clouds that they are for watering the earth—to take Bacon's examples—or 'that the solidness of the earth is for the station and mansion of living creatures.' The physical philosopher has it for his business to trace clouds to the laws of evaporation and condensation; to determine the magnitude and mode of action of the forces of cohesion and crystallization by which the materials of the earth are made solid and firm. This he does, making no use of the notion of final causes; and it is precisely because he has thus established his theories independently of any assumption of an end, that the end, when, after all, it returns upon him, and cannot be evaded, becomes an irresistible evidence of an Intelligent Legislator. He finds that the effects, of which the use is obvious, are produced by most simple and comprehensive laws; and when he has obtained this view, he is struck by the beauty of the means, by the refined and skillful manner in which the useful effects are brought about;—points different from those to which his researches were directed."

As the question, then, to which the cosmogony of Laplace proposes a solution, is a legitimate one, we shall proceed to consider some of the evidences by which the theory is supported.

1. *The nebular hypothesis furnishes a very simple explanation of the motions and arrangements of the planetary system.* In the first place, it is evident that the separation of a ring would take place at the equator of the revolving mass, where of course the centrifugal force would be greatest. These concentric rings—and consequently the resulting planets—would all revolve *in nearly the same plane.* It is evident also that the central orb must have a revolution on its axis *in the same direction with the progressive motions of the planets.* Again: at the breaking up of a ring, the particles of nebulous matter more distant from the sun would have a greater absolute velocity than those nearer to it, which would produce the observed *unity of direction in the rotary and orbital revolutions.* The motions of the satellites are explained in like manner. The hypothesis, moreover, accounts satisfactorily for the fact that the orbits of the planets are all nearly circular.

* Bridgewater Treatise, vol. ii, p. 180.

And finally, it presents an obvious explanation of the rings of Saturn. It would almost seem, indeed, as if these wonderful annuli had been left by the Architect of Nature, as an index to the creative process.

The argument derived from the motions of the various members of the solar system is not new, having been forcibly stated by Laplace, Pontécoulant, Nichol, and other astronomers. Its full weight and importance, however, have not, we think, been duly appreciated. That a common physical cause has determined these motions, must be admitted by every philosophic mind. But apart from the nebular hypothesis, no such cause, adequate both in mode and measure, has ever been suggested;—indeed none, it seems to us, is conceivable. The phenomena which we have enumerated *demand* an explanation, and this demand is met by the nebular hypothesis. It will be found, therefore, when closely examined, that the evidence afforded by the celestial motions is sufficient to give the theory of Laplace a very high degree of probability.

2. The fact that this theory of the genesis of the solar system may be extended to the binary and multiple systems among the so-called fixed stars, may be urged as no inconsiderable evidence in its favor. Unity, no less than variety, is characteristic of nature's works. All the diversified and apparently disconnected phenomena of the universe have their roots in a few general laws. Whatever, therefore, leads us higher in the process of generalization may be presumed to have some foundation in truth.

3. Numerous geological facts appear to harmonize with the hypothesis under consideration. Fossil organic remains, and their absence in the earlier rocks, both indicate that the temperature of the earth's surface was formerly much higher than at present, and that the decrease was slow and gradual. In places, for instance, where ferns do not now exceed three feet in height, those of former periods are found to have been from forty to fifty, or even seventy feet high. Now in regard to existing plants, it is well-known that their *number*, as well as their *size and luxuriance of growth*, gradually increase as we advance from high latitudes towards the equator. In both these respects a similar increase is observable in descending through the successive strata of the ancient world, until we reach the oldest Secondary rocks. These facts are doubtless to be referred to the same cause, namely, a gradual change of temperature. A comparison of fossil animals with those now existing, leads to a similar result. This high temperature of the earth's surface during the earlier stages of its physical history, is attributed by most geologists to a central heat,* which diffused itself through-

* A different theory in regard to the ancient high temperature of the earth has been developed by the celebrated Poisson. Starting with the fact—established be-

out the entire mass. That the interior of our planet is in a state of igneous fluidity, or has, at least, an extremely high temperature, is now very generally admitted. As we descend from the surface we find a regular increase of heat, varying indeed for different localities, partly no doubt, on account of the different conducting powers of the rocks which constitute the crust, but averaging about one degree for every fifty feet.* This has been ascertained from a great variety of experiments upon rocks and springs in mines, and upon the water issuing from Artesian wells. It is true, the depth to which man has been able to penetrate, is comparatively small; but assuming the same rate of increase to continue, it has been calculated that the point at which all known rocks would be in a state of fusion must be considerably less than one hundred miles from the surface, and possibly less than fifty. Reasons are not wanting for regarding this molten mass as the seat of present volcanic activity,† as well as the source of that great upheaving power by which not only mountains, but islands and continents have been elevated.

Geologists are generally agreed that there is conclusive evidence of the primitive igneous fluidity of the unstratified rocks of the earth's crust. Indeed it can scarcely be doubted that the solidification of the outer portions of our globe was a mere cooling by radiation from the surface. "There is no small reason," says Professor Hitchcock, "to suppose that the globe underwent numerous changes previous to the time when animals were placed upon it; that, in fact, the time was when the whole matter of

yond doubt by modern observations—that the solar system has a progressive motion in space, he supposes that in the sweep of its mighty orbit it has passed through regions of very different temperatures, and that the heat of former periods—the residuum of which is still found at great depths beneath the surface—was received *ab extra* while in a portion of space much richer in stars, and having therefore a higher temperature, than that through which it is at present passing. "The physical doubts which have reasonably been entertained against this extraordinary cosmical view, (which attributes to the regions of space that which probably is more dependent on the first transition of matter condensing from the gaseo-fluid into the solid state,) will be found collected in Poggendorff's *Annalen*, bd. xxxix, S. 93—100." Humboldt's Cosmos, vol. i. p. 165.

* See Proceedings of the American Association for the Advancement of Science, Tenth Meeting, Part II. p. 102.

† "The similarity of lava, wherever found, and the close agreement as to composition and physical characters of the basalt of ancient epochs and of that still bursting through and intersecting the walls of modern volcanoes, are proofs that all such eruptions have a common origin, and are due, as well as the accompanying physical phenomena of earthquakes, to forces acting on the still liquid portion of the earth." *Portlock's Rudimentary Geology*, p. 70.

The researches of Hopkins and Hennessy have led to different results in regard to the thickness of the earth's crust. See Trans. of the Roy. Soc. for 1839, p. 381; for 1840, p. 193; for 1842, p. 43; for 1851, Part II, p. 495. Also, this Journal for March, 1852, p. 271, and for May, 1853, p. 126. For "some suggestions in Explanation of the primitive Incandescent condition of the Earth and the other Planets," see Monthly Notices of the Roy. Astro. Soc. for January 13th, 1854.

the earth was in a melted and not improbably also in a gaseous state."* All this, it will be perceived, is in striking accordance with the Nebular Hypothesis.

4. Whatever may be the nature of the elevating force, it is not confined to our globe. The lunar and planetary mountains afford evidence of its action in the other members of our system—a fact which seems indicative of their common origin.

5. The spheroidal figure of the planets points to a great and significant fact in regard to their primitive constitution—the fact that they have *all*, at former epochs in their history, been in a liquid, if not in a gaseous state. That the polar flattening of the earth is not conclusive evidence of its primitive fluidity, has been affirmed, we are aware, by more than one writer of scientific eminence. If we suppose our planet to have been originally solid and perfectly spherical—its surface being entirely covered with water—the effect of its axial revolution would be the accumulation of water in the equatorial region, and a consequent recession from the poles. Sir John Herschel remarks,† that the gradual abrasion of these polar continents and their slow deposition in the deep equatorial ocean would eventually reduce the solid earth itself to the form of an oblate spheroid. Recently, however, H. Hennessy, an eminent mathematician, has subjected this hypothesis to the best of scientific scrutiny. The learned and original researches of this gentleman have shown that the ultimate ellipticity, in case the earth were at first a *solid* sphere, as supposed by Herschel, would be $\frac{1}{\frac{1}{3}\frac{1}{0}\frac{1}{1}}$, while that found by actual measurement is $\frac{1}{\frac{1}{2}\frac{1}{9}\frac{1}{3}}$ to $\frac{1}{\frac{1}{3}\frac{1}{0}\frac{1}{2}}$.‡ The theory of primitive fluidity may therefore be regarded as fully established. It is worthy of remark that the oblateness of Mars is much greater than would have resulted from its observed velocity of rotation, supposing the planet to have been originally fluid, homogeneous, and of spherical form. How is this anomaly to be accounted for? On the nebular hypothesis we have only to suppose that in the process of transition from the gaseous to the liquid and solid form, "the liquid surface of some planets was solidified before they could assume the figure appertaining to their velocity of rotation."§

6. The Nebular Hypothesis affords the most probable explanation of the phenomena of comets. Laplace supposed these bodies to have had their origin in portions of nebulous matter which had been left about the points of equal attraction between the sun and neighboring stars—the occasional preponderance of the solar influence causing portions of these outstanding nebulosities to enter our system from different quarters of the heavens. The orbits of such bodies would be very eccentric, and might have

* Religion of Geology, p. 22. † Outlines, Arts 226, 227. ‡ Mädler, p. 50, this vol.
§ Humboldt's Cosmos, vol. iv, (Bohn's Edition,) p. 427.

any degree of inclination to the plane of the ecliptic. Their motion, moreover, might be either direct or retrograde. Thus the great characteristics which distinguish the members of the cometary from those of the planetary system are necessary consequences of this theory of their origin.

"The attraction of the planets," says Laplace, "and perhaps also the resistance of the ethereal medium, ought to change several cometary orbits into ellipses, of which the greater axes are much less than the radius of the solar activity. It is probable that such a change was produced on the orbit of the comet of 1759, the greater axis of which was not more than thirty-five times the distance of the sun from the earth. A still greater change was produced in the orbits of the comets of 1770, and of 1805."*

7. The evidence afforded by the telescope in regard to the present physical constitution of the different members of the solar system appears to be confirmatory of the nebular hypothesis. If the sun and planets have condensed from a gaseous state, the former, not only on account of the comparative recentness of its formation, but especially because of its great relative magnitude would retain an intensely high temperature for an indefinite period after the planets had cooled down. *The present heat, therefore, of the central orb, and the existence of a very extensive gaseous envelope, together with the various phenomena of the solar spots, would result as consequences from the theory of Laplace.*

The Moon.—Although the diameter of the earth is nearly four times that of her satellite, the mountain elevations of the latter are nearly equal to those of the former. The cup-shaped cavities, which cover a great part of the lunar surface, are very different in appearance from volcanoes on the earth. The largest are from fifty to one hundred and fifty miles in diameter, and some are from three to four miles deep. In no case, however, is there any decisive evidence of *present* volcanic activity, though doubtless the phenomena justify the inference that at some remote epoch in the past history of our satellite, its crust has been agitated and shattered by up-heaving eruptive forces, compared with which the similar agencies in our own planet, at least those in operation during the historical period, sink into insignificance.

In contemplating these striking characteristics of the moon's surface—the traces of ancient igneous activity over the whole visible hemisphere, and the apparent absence of organization—it is natural to inquire—what is their physical import? If our satellite, in accordance with the nebular hypothesis, has solidified from a gaseous state, the earth having also undergone the same process, the latter, on account of its superior magnitude, would require a much longer period than the former to cool down from

* System of the World, Harte's Translation, vol. ii, p. 364.

a condition of igneous fluidity. The moon may therefore be farther advanced in its physical history than the earth. On this subject Mr. Nasmyth, of Manchester, England, who has devoted much time and attention to observations on the lunar surface, remarks as follows:—"Having in my travels, seen the actual results of volcanic action, extinct and active, I think I can comprehend what I observe on the moon, and trace the analogy where it is applicable. * * * I may express one of my most definite and strong convictions in a few words, namely, *that I do not believe there is one of the countless thousands of volcanoes, whose craters bespatter the lunar surface, in action, or which has been in action, for thousands of ages past.* I am vain enough to think I have got the right view of the true nature of volcanic action; a view which close observation of the phenomenon, in all its phases, has impressed on me; namely, *that volcanic action is an expiring phenomenon*, having for its cause and source great cosmical principles, quite independent from any mere chemical action; and, in that view, I consider molten lava, and the heat of volcanic action, to be nothing less than the residue of that *igneous* state through which *all* the planets have passed, in their cosmical history, from the earliest moment of their creation to the present time. And, in this view, if our globe be permitted to exist, in its present condition, for ages to come, volcanic action, as an active phenomenon will dwindle away, and finally cease to exist,—the solid crust of the earth so increasing in thickness as to prevent the issue of any of the yet remaining molten matter from its interior.

"The moon, from its small mass, and proportionally great surface, must have cooled down much more rapidly than the earth; and all have been dead, tranquil and silent, for countless ages, ere we had passed over our rampant volcanic era, of which our most tremendous modern volcanoes are but mole-hills in comparison."

Jupiter and Saturn.—What indications do the belts of Jupiter and Saturn afford in regard to the present physical condition of these planets? When our own planet was in a molten state, where was the water which now constitutes our oceans? It must necessarily have existed in the form of vapor, entirely surrounding the earth, and at a considerable distance from it. Nor could this water descend to, and remain permanently upon the surface, until long after the solidification of the crust by cooling. During this "pre-oceanic" period, our globe, it is likely, would have appeared to a distant spectator, very much as Jupiter and Saturn with their cloudy belts now appear to us. Now it has been justly remarked, that "the length of time which would be required for such enormous planets as Jupiter and Saturn to cool down from their original molten and incandescent condition to such a

temperature as would be fitted to permit their oceanic matter permanently to descend and remain upon the surface, *would be vastly longer than in the case of such a comparatively small planet as the earth.*"

Thus, in accordance with the nebular hypothesis, it would seem that Jupiter and Saturn, on account of their vast dimensions, still retain so much of their primitive heat as to produce the vaporous envelopes by which they are surrounded.

Variable Stars.—The well-known phenomena of variable stars, the explanation of which has greatly perplexed astronomers, is ingeniously accounted for, according to the nebular hypothesis, by Mr. A. D. Wackerbarth, as follows: "Suppose a nebula such as that from which the earth, sun, and planets, are supposed to have arisen, existing in space, such a nebula would probably be composed of elements more or less the same as those whereof our own planet is formed.* Some, indeed, of the latter we might suppose wanting, and others present which we possess not here; but on the whole, let us suppose that the chief elements of the earth are found in our nebula, which would thus form an immense spheroid of nebulous matter revolving round its own minor axis; or rather, if that matter were not quite homogeneously distributed, on an axis passing through its centre of gravity. We may suppose or not as we please, that this nebula has a nucleus, as many nebula appear to have, and many not to have, any such portion; but in the former case we must suppose some little difference in the constitution of the particles towards the centre, or position of the said nucleus. Our nebula, thus composed, may wander a longer or shorter period in space peacefully; but now let us suppose a disturbance such as that which broke up the nebulosity of the mass which forms the planetary system, and condensed it into separate globes. Such disturbance might come from without or from within; there are forces in nature to account for either. We have supposed all or many of the elements present; but in a nebulous form they would be in a finely divided state, and many of them, perhaps all the baser metals, have such affinity to oxygen, as when in that state, to take fire on coming in contact therewith; so that any cause, which would bring them and the oxygen into contact, would cause fearful explosions, and set the whole nebula in

* It is a remarkable fact that no *new* elements have been found in meteoric stones, which are now regarded as planetary bodies. "I would ask," says Humboldt, "why the elementary substances which compose one group of cosmical bodies, or one planetary system, may not in a great measure be identical? Why should we not adopt this view, since we may conjecture that these planetary bodies, like all the larger or smaller agglomerated masses revolving round the sun, have been thrown off from the once far more expanded solar atmosphere, and been formed from vaporous rings describing their orbits round the central body?" (Cosmos, vol. i, p. 120.) Mr. Wackerbarth extends this hypothesis in regard to the identity of the elementary substances to *stellar systems*.

ignition, condensing it into burning masses, each by the violent explosions casting out smaller fused and burning masses into space, to revolve as burning globes around itself, or the centre of gravity of the whole. But what would be the condition of one of these fused and blazing masses? Hydrogen is present as well as oxygen; and ignition must therefore immediately cause the production of water. An ocean is thus poured down on the incandescent globe, to be cast up again as steam into a damp atmosphere formed by the nitrogen and the watery vapor. Here it cools, and is again poured down in torrents on the glowing mass; and this process must continue until the globe has sufficiently cooled for the water to be able to rest upon its surface. Now, I imagine that the appearance of a globe in such a state viewed at a stellar distance, must be variable; that when the water is, in the form of steam, driven up into the atmosphere, the burning mass must glow with greater brilliancy; but when, the steam condensing, a boiling ocean is poured upon it, the violence of the conflagration must for a time abate, and thus the object assume a less brilliant appearance, until the fire has succeeded in reconverting the water into steam, and driving it up again into the atmosphere.

"Another circumstance may be mentioned as possible, namely, some bodies may be at present in some parts extinct; while other parts of them are yet fused and burning. Would not the revolution of such a body present the phenomena of a variable star?"*

8. *The Satellites.*—It is a remarkable coincidence in regard to the motions of the moon that her rotation is completed in a period precisely equal to that of her orbital revolution. The same is true of Jupiter's satellites, at least *some* of Saturn's, and probably, indeed, of all secondaries. Such coincidences are not to be ascribed to chance. Either, therefore, we must regard them as *ultimate facts*, or refer them to the operation of those primitive natural forces by which other phenomena of the heavenly bodies were produced. Few correct thinkers, we presume, would be inclined to adopt the former alternative. "The craving of the philosophic mind is for *explanation*, *i. e.*, for the breaking up of complex phenomena into familiar sequences, or equally familiar transitional changes, or contemporary manifestations."† There can be no doubt that the equality in question is due to the operation of known physical forces. Sir Isaac Newton's explanation,‡ it is well-known, assumes the original fluidity of the satellites. The attraction of the earth on the primitive fluid mass of the moon would produce an elongation of the hemisphere in the

* Monthly Notices of the Roy. Astr. Soc. for May 9th, 1856.

† Edinburgh Review, No. 175. ‡ Principia, B. iii, Prop. xxxvii, Cor.

direction of the primary. The force of gravity of this tidal elevation would maintain the greatest axis in the direction of the attracting body, thus producing the observed coincidence. The same applies to the other secondary planets.

This equality, we believe, is best explained by the nebular hypothesis. Newton's explanation pre-supposes the two motions to have been originally so adjusted as to differ *very little* from exact coincidence. But if the moon once existed in a state of vapor, its volume was much greater than at present, and the gravity of particles at its surface proportionally less; while from the fact of their gaseous condition they would yield with the greatest facility to any force impressed upon them. The equality between the two motions might thus become established long before our satellite had contracted to its present dimensions.

But why, it may be asked, should this isochronism obtain universally in the subordinate systems, while there is not the least approximation to it in the case of any primary planet? It has been suggested that "the integrity of the motions of the ring, when it resulted in satellites, may have arisen from the comparative maturity of the system—then approaching the close of the first epoch in its history."* Without entering here into any discussion of this question, we may remark that in the case of the secondary planets, *the epoch of solidification would evidently be reached, and consequently the acceleration of the rotary velocity arrested, at a comparatively early period of their existence.* "The motions of the three first satellites of Jupiter present a phenomenon still more extraordinary than the preceding; which consists in this, that the mean longitude of the first, minus three times that of the second, plus twice that of the third, is constantly equal to two right angles. There is the ratio of infinity to one, that this equality is not the effect of chance. But in order to produce it, it is sufficient, if at the commencement, the mean motions of these three bodies approached very near to the relation which renders the mean motion of the first, minus three times that of the second, plus twice that of the third, equal to nothing. Then their mutual attraction, rendered this ratio rigorously exact, and it has moreover made the mean longitude of the first, minus three times that of the second, plus twice that of the third, equal to a semi-circumference. At the same time, it gave rise to a periodic inequality, which depends on the small quantity, by which the mean motions originally deviated from the relation which we have just announced. Notwithstanding all the care Delambre took in his observations, he could not recognize this inequality, which, while it evinces its extreme smallness, also indicates, with a high degree of probability, the existence of a cause which makes it disappear. In our hypothesis,

* Dr. Nichol.

the satellites of Jupiter, immediately after their formation, did not move in a perfect vacuum; the less condensible molecules of the primitive atmospheres of the sun and planet would then constitute a rare medium, the resistance of which being different for each of the stars, might make the mean motions to approach by degrees to the ratio in question; and when these movements had thus attained the conditions requisite, in order that the mutual attraction of the three satellites might render the relation accurately true, it perpetually diminished the inequality which this relation originated, and eventually rendered it insensible. We cannot better illustrate these effects than by comparing them to the motion of a pendulum, which, actuated by a great velocity, moves in a medium, the resistance of which is inconsiderable. It will first describe a great number of circumferences; but at length, its motion of circulation perpetually decreasing, it will be converted into an oscillatory motion, which itself diminishing more and more, by the resistance of the medium, will eventually be totally destroyed, and then the pendulum, having attained a state of repose, will remain at rest forever."*

9. *The Asteroids.*—Our data perhaps may not yet be sufficient to afford a satisfactory explanation of the phenomena of the asteroids. The question, however, in regard to their origin, and the bearing of this inquiry on the cosmogony of our system, are subjects of more than ordinary interest. The following facts appear to indicate the most plausible theory of their origin:—

(1.) These bodies occupy a chasm—observed before their discovery—in the order of the planetary distances; an order which indicated the existence of a *single planet* where *fifty-eight* have already been detected.

(2.) The members of the group are characterized by certain peculiarities which are doubtless indicative of an intimate mutual relationship. They are extremely diminutive in size; the volume of the largest probably not exceeding the ten-thousandth part of that of the earth. The eccentricities and inclinations of their orbits are generally much greater than those of the other planets. Their orbits interlace. "The strongest evidence," says D'Arrest, "of the intimate connection of the whole group of small planets appears to be, that if the orbits are supposed to be represented materially as hoops, they all hang together in such a manner that the entire group may be suspended by any given one." The occasional rapid variation in the apparent magnitude of several members of the group—changes not attributable to variations of distance—would seem to indicate some peculiarity in their physical constitution.

(3.) The asteroids already discovered amount to one for about every six degrees of longitude. Their number appears to *in-*

* Harte's Translation of Laplace's System of the World, vol. ii, p. 367.

crease with the *decrease* of their apparent magnitude. In all probability hundreds of these bodies will have been discovered by the close of the present century; and the existence of immense numbers too small to be detected, cannot reasonably be doubted. In fact, the zone is even yet almost entitled to the appellation of a *primary planetary ring.*

(4.) Should the rings of Saturn, (now regarded by some astronomers, as fluid,) from any cause whatever become separated into parts, and collect about distinct nuclei, a ring of *secondary asteroids* would be formed, analogous to the zone between Mars and Jupiter.

(5.) It has been affirmed by an eminent astronomer that the rings of Saturn are sustained by the direct action of the satellites; that no other planet of the system has such an arrangement of secondaries as to secure the stability of a ring; and that the only place in the solar system where a primary ring would be long sustained, is the region occupied by the small planets.

(6.) The amount of perturbation in this point of the system is extraordinary in consequence of its proximity to the enormous mass of Jupiter.

Now according to the nebular hypothesis each of the principal planets originally existed as a gaseous ring. The observed order would require such an annulus between Mars and Jupiter, and this is the precise situation in which a ring would be the longest sustained by the exterior planets. Upon the breaking up of the ring, however, *a zone of small planets would naturally be formed* unless some one portion of the vaporous mass should have a preponderating influence so as ultimately to absorb all the rest. In short, it is believed that the nebular hypothesis will explain the various phenomena of these bodies, which without it seem inexplicable.

10. *The Zodiacal Light.*—Cassini regarded the zodiacal light as produced by an innumerable multitude of small planetary bodies revolving in a ring about the sun. This hypothesis was generally accepted until 1855, when another was proposed by the Rev. George Jones, of the United States Navy. Mr. Jones thinks his own observations of the phenomena wholly incompatible with the hypothesis of a nebulous ring *with the sun for its centre*, and also with that of a nebulous planetary body revolving round the sun. He is led, therefore, to the alternative of a *nebulous ring round the earth, interior to the moon's orbit.* This theory has been favorably received by the astronomers of our country. Objections of some weight, however, have been urged against it by Pres. F. A. P. Barnard,* of the University of Mississippi, and it will not perhaps be generally adopted without further confirmation. But it is generally admitted by the advocates

* This Journal for March, 1856.

of each hypothesis that the appearance is produced by a *continuous zone of infinitesimal asteroids.* Now if the abandoned rings of either the *solar* or the primitive *terrestrial* atmosphere contained any matter of too volatile a nature to coalesce in the formation of solid planetary bodies, such molecules ought to exhibit the appearance of a flattened ring, of a vapory or nebulous form, like that actually presented by the zodiacal light. Whether we regard it, therefore, as a primary or secondary ring, the theory of Laplace affords the most plausible explanation of its origin.

11. *The Rotations of the Planets.*—Is there or is there not any bond of union between the periods of rotation of the different members of our system? That these rotations should be mere isolated facts, independent of all order and relationship, seems, to say the least, extremely improbable. Even in the process of crystallization the atoms of matter arrange themselves in regular mathematical forms; the chemical combination of elements occurs only in definite proportions; the *orbital* motions of the planets are governed by invariable laws; in short, order and harmony appear to characterize all the operations of nature around us, or in the language of an ancient philosopher, "God geometrizes." Is there any probability that we have an exception to this general prevalence of law, in the *rotations* of the heavenly bodies?

The first attempt, it is believed, to develop a connection between these apparently independent elements, was undertaken by the writer in 1839. The result of these researches was the discovery of an interesting planetary harmony, which, in 1849, was communicated to the American Association for the Advancement of Science.* It is not claimed for this Analogy that it has been fully established as a physical law. It appears, however, to harmonize too closely with the known elements of the solar system, to be regarded as merely an accidental coincidence. Now the immediate dependence of one of the elements of this analogy—viz., the diameter of the sphere of attraction—on the nebular hypothesis, is obvious. It is evident also that coincidence between the periods of rotation and revolution is a necessary characteristic of planetary rings revolving around a central mass, and that on the transformation of a ring into a planet the same equality would obtain when the *diameter* of the latter was equal to the *breadth* of the former. Any further condensation, however, would *increase* the rotary velocity, and thus shorten the period of axial revolution. Hence *the ratio of the angular velocity of rotation to that of orbital revolution, or what is the same thing, the number of a planet's days in its year, would be a function of the diameter of the sphere of attraction.*

12. *The distribution of the satellites.*—Another interesting con-

* See Proceedings of the Am. Assoc., for 1849, p, 207.

sideration may here be suggested. Planets of greater magnitude and having greater spheres of attraction would probably throw off the greater number of rings. An explanation may thus be given of the striking fact in the constitution of the solar system, that the exterior planets have numerous satellites. Saturn, which has the greatest sphere of attraction, has the greatest number of satellites; Uranus, the next in order in this respect, has the next greatest number; while Jupiter, which is the *third* in regard to the former, is *third* also in the latter. Mars, it is true, has a greater sphere of attraction than the earth; it may be remarked, however, that owing to the small size of the planet, and its proximity to Jupiter, a satellite would not be *stable* unless very near the primary.

13. *Recapitulation—Simplicity of the Nebular Hypothesis.*— When the number and variety of the phenomena explained by the nebular hypothesis are duly considered. the weight of evidence in its favor must be admitted, we think, by every unbiassed mind. The rotation of the sun; the unity of direction of the planetary motions; the approximate coincidence of the planes in which the planets move, with the plane of the solar equator; the general agreement between the direction of the axial and orbital revolutions; the small eccentricities of the planetary orbits; the rings of Saturn; the central heat of the earth; the oblate form and primitive fluidity of the planets; the origin of comets; telescopic revelations in regard to the physical constitution of the sun, moon, and some of the planets; the phenomena of variable stars; the equality between the periods of rotation and orbital revolution of the satellites; the extraordinary phenomena presented by the first three satellites of Jupiter; the zone of asteroids between Mars and Jupiter; the zodiacal light; the analogy between the periods of rotation of the primary planets; the distribution of the satellites;—all, as we have seen, are accounted for by the nebular cosmogony, while for many of them at least, no other explanation has ever been offered. Adopting this hypothesis, all the motions of the solar system are derived from a *single impulse* communicated by the Creator to the primitive nebula: rejecting it, each motion of every member demands the *separate* operation of his power. Now, "if there be two modes of explaining any phenomenon of nature, then, *cæteris paribus*, that is the most probable which is the most simple. For by what we observe in the creation around, we are forced upon the conviction that the Almighty acts in this respect with that *economy* of creative energy, which, although infinitely more perfect in its degree, has nevertheless its visible type in that husbandry of *our* resources, that disposition to economy in *our* efforts, which impels us always to avail ourselves of the simplest possible means of effecting all that we wish to do.

"Thus, when in reasoning upon any hypothesis, we are forced back upon secondary causes, it is sound philosophy to judge of the probability of that hypothesis, according to the simplicity or complication of the causes to which we are thus compelled ultimately to refer it. If, for instance, there be two hypotheses, by one of which we shall be compelled to fall back upon a double operation of the hand of the Almighty, whereas the other resolves it into a single effort of his will, then is the latter hypothesis, according to the analogy of nature, more probable than the former, and that infinitely."

Objections to the Nebular Hypothesis.

1. *The Satellites of Uranus.*—It has been objected to the nebular hypothesis that it cannot be reconciled with the retrograde motion of the satellites of Uranus. We reply that in every instance, so far as we know, the motions of secondary planets are performed in planes nearly perpendicular to the axis of the primary, but that in no case, with the single exception of Jupiter, is the axis of a primary planet nearly perpendicular to the plane of its own orbit. The earth's axis is inclined more than twenty-three degrees; that of Mars, as well as that of Saturn, nearly thirty; while that of Venus, if we may rely on the observations of Schroeter, is inclined more than seventy degrees. Now if this amount of inclination does not invalidate the hypothesis in question, ought the greater one of one hundred degrees—which gives the satellites of Uranus their backward motion—to be regarded as wholly inconsistent with it? These inclinations, as well as the slight eccentricities of the orbits, and their deviations from the plane of the sun's equator, may be ascribed partly to irregularities in the density and temperature of the planets in a state of vapor, and to the operation of molecular forces during the process of condensation. The following suggestion of Laplace is also worthy of note:—"If any comets have penetrated the atmospheres of the sun and planets at their formation, they must have described spirals, and consequently fallen on the bodies, and in consequence of their fall, caused the planes of the orbits and of the equators of the planets to deviate from the plane of the solar equator."* "The probabilities of the case," says Professor Nichol, "certainly favor the idea, that it [the anomalous motion of the Uranian satellites] is a *disturbance*, an *interference* with the *order* of the system, by some *foreign* law or occurrence."†

But while the hypothesis requires *in general* that the direction of the planetary rotations, as well as that of the orbital motions of their satellites, should be the same as that of their revolution

* System of the World, Harte's Translation, vol. ii, p. 364.

† Thoughts on some Important Points Relating to the System of the World. First American Edition, p. 109.

round the sun, an *opposite* direction is not, as some have supposed, incompatible with it. Perhaps, indeed, it is not more improbable than a permanent ring, like that of Saturn, or a zone of small planets like the asteroids. We have supposed that on the breaking up of a nebulous ring its matter collected into several masses, the orbits of which were not greatly different, and that they revolved separately during an indefinite period, but ultimately united, except in the case of the asteroids, in *one* planetary body. Those primitive masses, owing to perturbations, &c., would meet in different directions, and it is by no means impossible that the circumstances of the collision and coalescence of the nebulous fragments may have been such as to overcome the natural tendency to rotate in the direction of the orbital motion.

Again: the form of the primary ring may have influenced the original *direction* of the planet's rotation, and hence that of its *secondary* rings and the resulting satellites. If the thickness of a ring was greatest in the direction at right angles to the plane of its motion, or if the diameter of its mass when it first assumed the form of a gaseous planet, was greatest in that direction, the plane of rotation would probably, for obvious reasons, be inclined to that of the orbit; the amount of inclination depending upon the ratio of the diameters.*

2. *The Revelations of Lord Rosse's Telescope.*—The opponents of the nebular hypothesis affirm that it derived its chief support from the supposed existence of irresolvable nebulæ; that is, of widely diffused vaporiform matter not yet aggregated into stars, but slowly undergoing the process of condensation. Such, until 1846, was believed to be the constitution of a large proportion of those mysterious objects. The great nebula in Orion, for instance, was thought to present decided indications of irresolvability. Lord Rosse's telescope, however, has shown it to consist at least in some portions, of minute stellar points, in extremely close proximity; in the language of Dr. Nichol, "every wisp, every wrinkle, is verily a sand-heap of stars." This discovery has been considered by some as satisfactory evidence that all nebulæ are resolvable. The sublime conception of Laplace, it has been dogmatically affirmed, can hereafter be regarded as but a "splendid vision." We reply that the *principal* evidence in favor of the theory, is that afforded by the phenomena of the solar system itself, and that *this* would not be in the least invalidated should every nebula within the reach of the telescope be resolved into stars. But no results hitherto obtained justify the conclusion that all nebulæ are composed of stars. A large proportion remain still unresolved, even under the highest power of

* This idea has also been suggested by an anonymous writer in the Westminster Review.

the "Parsonstown Leviathan." As this, however, is a point much insisted on by the opponents of the nebular cosmogony, we shall endeavor to place it in its proper light.

The hypothesis that certain nebulæ consist of cosmical vapor gradually condensing around stellar nuclei, was proposed by Sir William Herschel; that of the genesis of the solar system from a primitive nebula was suggested by Laplace. The former resulted from a critical examination of the nebulæ themselves; the latter, from a philosophical discussion of *the phenomena of the solar system*. Laplace, it is true, adopted the Herschelian theory of nebular matter, and regarded it as highly favorable to his own hypothesis. Popular writers of the present day, however, look upon the former as constituting the entire ground of Laplace's speculations. "The theory," says a distinguished author,* "proceeds on the assumption of the existence and wide diffusion of a nebulous fluid." The achievements of Lord Rosse's telescope are accordingly claimed as almost decisive evidence against Laplace's cosmogony.

Now if the mere fact that we have no certain knowledge of the *present* existence of nebular vapor warrants the inference that the *primitive* condition of the solar system was not gaseous, by parity of reasoning it would follow that unless some of the *members* of the solar system are *now* in a fluid state, our own globe has always been *solid*. But the *form* of the earth proves its ancient fluidity; and so, in like manner, various phenomena of the planetary system indicate a primitive nebular state.

When it is remembered, moreover, that the analogy of the solar system is decidedly against the assumption that the different appearances of the stars are owing to a gradation of distances; that portions of our own sideral cluster, the milky-way, *have never been resolved;* and that while Lord Rosse's telescope has separated particular *parts* of some nebulæ into stellar points, it has left other parts *unresolved*, and revealed very faint nebulous ramifications which *cannot* be composed of stars, unless we regard the components as extremely minute, the nebular hypothesis does not appear to have been materially weakened by Lord Rosse's discoveries.†

Finally, we may remark that comets and the zodiacal light *demonstrate* the existence of cosmical vapor similar to that which the theory assumes.

* James Buchanan, D.D., LL.D.

† "Regarding the *nebulæ* as spherical in form, and not as vastly long vistas foreshortened by having their ends turned towards the earth—which would be improbable, seeing there are two of them close together—the brightness of objects in their nearer portions cannot be much exaggerated, nor those in its remote, much enfeebled by difference of distance. It must, therefore, be an admitted fact that stars of the seventh and eighth magnitudes and irresolvable nebulæ may coëxist within limits of distance comparatively small, and that all inferences in regard to relative distance drawn from relative magnitudes must be received with caution."—Bartlett's Spherical Astronomy, p. 222.

3. *The alleged atheistic tendency of the Nebular Hypothesis.*—The most prominent objection to the nebular hypothesis is its alleged *atheistic tendency.* It has been characterized as an attempt to exclude God from his own universe, by substituting *natural law* for his direct agency in the work of creation. The force of this objection must evidently depend upon the question—What are we to understand by the "laws of nature?" As the question is one in regard to which much misapprehension appears to have existed, its brief consideration may not be out of place.

In the nebular theory, the process of condensation, the separation of rings and their conversion into spheroids, the acceleration of rotary velocity, solidification, &c., are supposed to have occurred in accordance with the known laws of gravitation, centrifugal force, cohesion, and chemical combination. The hypothesis does *not* assume, however, that these laws were self-originated and independent material forces. It has no conflict with Divine revelation. It assumes the existence of chaotic matter—has nothing to do with its origination. Its advocates therefore can consistently grant that matter was created by a Being of infinite power, and that the *quantities* of the various elements were determined, their collocations arranged, and their respective properties appointed by a Being of infinite wisdom. *Laws of nature are formal expressions of the ordinary modes in which the Divine will is manifested in natural phenomena.* McCosh* specifies three different senses in which the phrase is understood, but they are all embraced in the preceding definition. "A law never acts;" but it invariably points to an intelligent agent or designer. Gravitation, for instance, is neither an essentially inherent property of matter, nor an absolute *cause* of motion. The Newtonian law by which it is expressed, so far from being independent of an All-wise and Omnipotent Author, is simply the rule by which the Creator governs the material universe. The assertion therefore that this law "is probably the only efficient principle of the creation of the physical world, as it is of its preservation,"† is obviously absurd.

The fact then of the existence of a *law*, necessarily implies the existence of a *law-giver;* hence the objection which we have stated is destitute of foundation. It must follow also as a necessary consequence that the nebular hypothesis is utterly *incompatible* with the very system to the support of which a false philosophy has attempted to pervert it. Moreover, if the power of the Deity is manifested in accordance with a uniform system—a "law of nature"—in *sustaining* and *governing* the material universe, why should it be regarded as derogating from his perfections to suppose the same power to have been exerted in a similar way in the process of its formation?

* On the Divine Government, B. II., chap. i, sec. 1.
† Pontécoulant.

But some writers, among whom we may mention in particular the author of the "Vestiges," have attempted to connect the nebular hypothesis with the Lamarckian theory of development. Each of these hypotheses, however, is complete in itself. The arguments which lie against the latter have no logical bearing whatever against the former. It is not necessary to our purpose therefore to discuss the Lamarckian theory; this has been ably done by Miller, in his "Footprints of the Creator;" Buchanan, in his "Modern Atheism;" and by other distinguished writers on both sides of the Atlantic. We have no hesitation, however, in affirming that its complete refutation leaves the nebular hypothesis untouched, and that the demonstration of the latter would afford no evidence of the truth of the former. To regard them as interlinked, dependent, and essential parts of a great atheistic scheme, is to mistake entirely their mutual relationship.

The foregoing are, we believe, the most weighty objections that have been urged against the nebular hypothesis. The first, or that derived from the retrograde motions of the satellites of Uranus, has doubtless the greatest force; the most eminent astronomers, however, have not deemed it insuperable.

But if we admit, in general, the truth of the nebular hypothesis, the question still remains whether the special form of it proposed by Laplace is that which obtained in the formation of our system, or whether certain modifications ought not to be admitted. Did the chaotic matter, for instance, advance with regularity through all the stages of condensation, or were its contractions sudden and violent? The former supposition, which is that of Laplace, has been generally adopted by writers on the subject; but the latter, as has been remarked by Professor Nichol, seems more in harmony with the known operations of nature around us. When gases pass to the liquid form it is not generally by gradual condensation: on the other hand, such changes are characterized by rapid and energetic action. The same is true, at least in some cases, when bodies pass from a liquid to a solid state.

It will be seen, we think, from the foregoing discussion, that the confirmation of the nebular hypothesis would tend to reduce the apparently isolated phenomena of the solar system to the domain of law, and show that, in a cosmical point of view, the material world, in each of its diversified operations, exhibits a *uniformity of action*, adapted to develop our intellectual powers, and direct our thoughts to the great First Cause—the ever-present, all-sustaining Governor of the Universe.

Bloomington, Indiana, 1860.

ART. XVI.—*On a new Theory of Light, proposed by John Smith, M.A.;* by OGDEN N. ROOD, Professor of Chemistry in Troy University.

SEVERAL months ago, when attempting, by means of a revolving disc, to measure the time occupied by the explosion of small charges of gunpowder, the following observation was made: The flame of a burning-fluid lamp was viewed through a rotating disc provided with four radial slits, and it was found that a certain rapidity of rotation caused the lower part of the flame to assume a green hue, while by a diminished rate the whole flame was colored deep purplish red; a lower rate gave a violet tint, alternating with pure white.

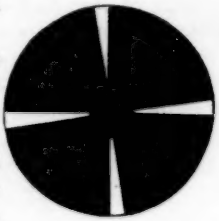
It was evident that these appearances depended much on the state of the eye, for they often could be perceived only after it had become a little fatigued by the blinding effect consequent on the comparatively slow succession of the impressions of light; in addition to this, I found that my colleague, Prof. Vincent, though recognizing the green tint, was unable to perceive the red hue, with which my eyes at that very moment were dazzled.

I considered these appearances, therefore, as *subjective*, and laid the matter aside for future experiment. As, however, similar phenomena have been observed in England, and have been thought to have not only an objective existence, but to be worthy materials on which to build a new theory of light, it may not be amiss to enter into a slight examination of their nature.

In Mr. Smith's experiments, of which I have been able to obtain only the account given in the March number of this Journal, bright white light is allowed to act on the eye during a certain fraction of a second; it is succeeded by shadow or darkness, which lasts also during a certain short interval of time, when the operation is repeated anew, &c.

This pulsation of light and shade the author effects in a variety of ways: the result is color—a yellowish green, purple, pink, &c. Fechner, to whom we are indebted for extensive researches on sight, several years ago observed that white discs having black spiral figures painted on them, when set in rotation exhibited colors which he considered subjective.*

1.



That these colors are really subjective, the following simple experiment may serve to show: A blackened disc nine inches in diameter, was cut with four slits of the shape seen in the wood cut; the width of the slits at the circumference was $\frac{5}{16}$ of an inch; the disc was made to rotate before a bright cloud.

* Pogg. Ann. vol. xlv, p. 227.

A rate of ten revolutions per second caused the cloud in a short time to appear of a deep red color, having in it a tinge of purple, or, according to Mr. Smith, the disc transmitted pink light; it was now viewed through a plate of orange-tinted glass. Previous experiment with a small telescope provided with a micrometer, and a flint glass prism, had shown that this orange colored glass readily transmitted the red, orange, yellow, and a portion of the green, but that it was opaque to the blue and violet rays; it was therefore fairly to be expected that if the disc was really transmitting red light, the plate of glass would do the same. The result was different; through the glass the disc appeared of a bright greenish-blue color. This experiment is very easy to make, and the effect is brilliant.

Plates of glass of other tints were now employed; the results are given below.

Medium.	*Tints of the Disc*
Yellow glass,	Violet,
Green "	Green, neutral, or faint red,
Red "	Red, neutral, or faint green.

As the green glass was nearly opaque to red rays, the effect of its use ought to have been darkness. It is evident by an inspection of this table that the disc really transmitted *white*, and not red light, which becoming colored by its passage through the plates of glass, induced in the retina, from time to time, the sensation of the complementary tint, more or less mingled with the *original* impression.

Having now shown that, contrary to Mr. Smith's supposition, the light transmitted by the revolving disc is really white, let us notice some of its effects on the eye.

For this purpose I caused perforated discs to revolve at uniform rates by means of clock-work; the arrangement being similar to that employed by Plateau. A blackened disc five inches in diameter and perforated with four slits 7° 12′ in the width was set in rotation, and the bright sky viewed through it; the eye of the observer being immediately behind the disc. With a rate of 11½ revolutions per second, the appearance of the window was as in fig. 2; a central spot was colored bluish-green, the rest of the field was purple, or reddish purple, according to the state of the eye. The green spot remained always in the axis of vision, and moved with each change of it. With the exception of fluctuations in the outline of the spot, this appearance remained tolerably constant, as long as the rate of revolution continued the same. The spot or shadow was fringed with a narrow, faint blue border, indicated by the dotted line.

2.

Upon increasing the rate of revolution, the bluish-green spot expanded into an irregularly shaped ring, and continued to expand, filling the field, till the rate had become as high as 15 rotations per second, when often the field for an instant became of a greenish tint which was succeeded by a bluish tint; upon increasing the speed this also vanished. Still higher rates cease to produce any of these peculiar effects on the eye.

3.

Upon slowly reducing the rate to 9 revolutions per second, the green spot contracted in dimensions, and assumed a yellow tint, while the field often became at the same time tinted deep crimson. With a rate still lower, the appearance of the field is variable and the tints flickering; it assumes sometimes a purple, a yellow, or a yellowish green tint.

This experiment I repeated a great number of times, with the same general result, and though it sometimes happened that the eye became insensible to these colors, from repetition, momentary rest in darkness restored this power for a short time. Thus it occurred that the tints were sometimes seen with great distinctness, while at others they could hardly be distinguished.

Upon a dark cloudy day to produce these effects it was found necessary to increase the width of the slits to 20°; from whence it was manifest that lack of intensity in the light might be made up by its longer duration.

It would appear then, from these experiments, that light from a bright cloud, if allowed to act on the eye repeatedly during from $\frac{1}{4\frac{1}{3}\frac{1}{0}}$ to $\frac{1}{7\frac{1}{5}\frac{1}{0}}$ of a second, developes subjective colors; that, however, the development of the subjective tint is dependent not so much on the length of time which the eye is exposed, as upon the interval of *rest* or shade which follows each exposure, may be shown in the following manner: In the experiment where with $11\frac{1}{2}$ revolutions a reddish purple was produced, the exposure lasted $\frac{1}{5\frac{1}{7}\frac{1}{3}}$ of a second; the interval of rest or shade was $\frac{1}{5\frac{1}{0}}$ of a second; now a disc was cut similar to fig. 1, but having eight slits, each 7° 12′ in width, when it was found that 5·5 instead of 11·5 revolutions per second produced the purplish-red tint; here the exposure was twice as long, but the interval of rest or shade nearly the same. With sixteen slits, $2\frac{3}{1\frac{1}{0}}$ revolutions produced the same tint, the exposure being of course four times as long, but the interval of rest nearly the same. Determinations of the length of this interval are given below:—

Length of the intervals of shade required for the production of

Blue,.....$\frac{1}{6\frac{1}{5}}$ sec.
Purplish-red,.....$\frac{1}{5\frac{1}{0}}$ "

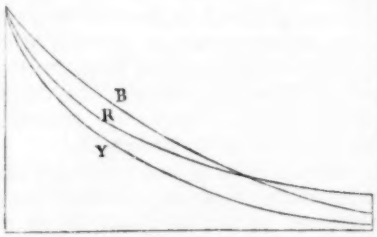
To ascertain exactly what portions of these intervals elapse

before these tints make their appearance, or how long the tints are actually seen by the eye, is another matter, and would no doubt require an elaborate series of experiments, though it would seem probable that at least half of the time of the above given intervals passes before the subjective color makes its appearance.

The table does not apply to the axial portion of the retina, which is almost always differently affected. That the change in susceptibility in the retina is progressive outwards, is shown by the gradual expansion of the green ring; that it varies from second to second, is seen in the fluctuations of the outline of this ring.

The occurrence and sequence of these subjective colors may easily be explained by supposing that during the interval of rest or shadow the action of the yellow rays diminishes more rapidly than that of the red, the red more rapidly again than that of the blue. If this takes place as indicated by the curves below, it is easy to understand the production of the tints, for if the moment after the blue has been developed white light be again presented to the eye, it is evident that neither purple nor red will be seen, sufficient time not having been allowed for their production.

4.



The same reasoning applies to the axial portion of the retina, which, owing to its greater sensibility to such impressions of light, requires a somewhat longer interval of rest before the reaction occurs.

If the impression be too strong, that is, if the light be too bright or the eye too long exposed to it, these peculiar effects are not observed, and during such short intervals of rest as $\frac{1}{5}\frac{1}{6}$ or $\frac{1}{6}\frac{1}{3}$ of a second the white clouds seen through the discs suffer no change in tint; but if a blackened disc twelve inches in diameter be cut as seen in the figure, with an aperture of 30°, and made to rotate before a white cloud at a rate of only one revolution per second, the eye placed as near it as possible, will most distinctly see, in the interval of darkness, an image of the sky, of a bluish-green tint. Whence it follows that an exposure of the eye to white light lasting $\frac{1}{1}\frac{1}{3}$ of a second, induces in it for a considerably longer time the sensation of this color.

5.

In general these experiments seem to point out that after momentary exposure to white light subjective colors are induced in the eye, whose tint and duration are dependent on the strength of the impression received, as well as upon the length of time allowed for rest; these sensations of color apparently having a relation to the colors observed after looking at the sun, similar to that which a temporary disorder bears to a chronic affection.

Troy University, March 20, 1860.

ART. XVII.—*On the Meteor of November* 15th, 1859; by Prof. H. A. NEWTON, Yale College.

IN the forenoon of November 15th, 1859, about half-past nine o'clock, a meteor or meteorite was seen to descend towards the earth by many persons at widely different places. Throughout the southern part of New Jersey a tremendous explosion was heard immediately after its appearance. Mr. Benjamin V. Marsh of Philadelphia has published in the Journal of the Franklin Institute, a very valuable collection of statements of persons who saw it from various positions, from Newburyport, Mass., to Petersburg, Va. In the January and March numbers of this Journal Prof. Loomis has also given a selection from the accounts of several persons who were so fortunate as to witness it.

The physical circumstances attending its passage through the atmosphere, its explosion, and its entire or partial combustion are of interest and the accounts referred to are of great value in studying them. My present purpose, however, is to determine as accurately as possible the *path* and *velocity* of the meteorite. The result of my investigation has been to establish almost beyond a doubt the conclusion, that *this body was not a member of the solar system but came to us from the stellar regions.*

In several instances have the paths of meteors or shooting stars been determined and velocities been computed which would necessitate with respect to them the same conclusion. But in general the data are so vague that the result lacks the certainty which is afforded by the meteor of November. This is especially true of the velocities given by M. Petit of Toulouse. In not a single instance have the results given by him seemed to me worthy of confidence. I have thought, therefore, that a careful discussion of this meteor was desirable.

The observations from Alexandria, Va., and New Haven, Ct., seem for several reasons the best for determining its path through the atmosphere. Mr. Marsh quotes the following from a letter of Mr. Caleb S. Hallowell of Alexandria.

"Abram Martin, a student particularly well qualified for such observations, was fortunately, standing perfectly still with the

meteorite in full view during its entire visibility, and he seems to have completely daguereotyped its path and appearance on his mind. By my direction he made at the time a careful note of the spot where it fell, and as to the point in the heavens from whence it originated, he, on two *independent* and *distinct* occasions pointed the telescope of the theodolite very nearly to the same spot, as given below.

	1st obs'n.	2nd obs'n.	Mean.
Apparent altitude at moment of appearance, .	40°	39½°	39¾°
Azimuth, " " "	N. 83 E.	N. 85 E.	N. 84 E.
Apparent altitude at moment of disappearance, behind houses, ...	11°	10°	10½°
Azimuth, " " " .	N. 77 E.	N. 76 E.	N. 76½ E.

The time of flight was estimated at two seconds. The determination of the point from which it originated was made some weeks after the 15th of November.

In New Haven it was seen by Judge W. W. Boardman, who was standing at the time in the west door of the Exchange Building, Church Street. He was conversing with a friend, and looking S.E., when his attention was caught by the meteor down the street.

It shot obliquely down across the street and disappeared behind the cupola of the Catholic church, which is 700 or 800 feet distant. The course of the street is about S. 29° W. He says it must have been seen as far east as the line of the street. Mr. E. C. Herrick, from conversation with Judge Boardman some days after the meteor was seen, concluded that the most probable angle of its path with the vertical was 33°, though it might vary from this several degrees. This inclination was obtained, first by calculation from the apparent altitude at the moment of appearance, and second, by holding up a rod at the inclination which Judge Boardman said equalled that of the meteor's path and measuring this inclination. The perpendicular lines of the buildings would help to impress on his mind the direction. The point of disappearance was, in azimuth S. 35° 34′ W., at an altitude of about 6°. The time of flight was estimated as one second, or between one and two seconds.

The position of Judge Boardman was N. lat. 41° 18′ 18″, W. long. 72° 55′ 10″. That of Alexandria is N. lat. 38° 49′, W. long. 77° 4′. There are indications which go to show that the path of the meteor was not a straight line, especially near its lower extremity. But the part seen by Judge Boardman must have been very nearly straight, and may be so considered without serious error. The plane passing through New Haven, and the point of the heavens S. 35° 34′ W., altitude 6°, and making there an angle of 33° with the vertical, meets the plane passing through Alexandria and the two points given in the last column of the table of Mr. Hallowell in a straight line, which I shall consider the path of the meteor. This line cuts the earth

in W. long. 75° 10′ 30″, and N. lat. 39° 13′, a place a mile north of False Egg Island Point, just on the eastern shore of Delaware Bay. The azimuth of the point from which the meteor would approach the earth, at that place, was N. 88° 55′ E., and the altitude was 49° 35′.

Observations at New York, and also in the neighborhood of Dennisville, N. J., prove that the azimuths of the points of disappearance at each station are quite exact. The only important question is, whether the inclinations reported are reliable. To test their general accuracy, I have collected such other observations as seem at all reliable, and can be compared with them.

The inclination of 33°, at New Haven, is confirmed by observers at Salem, Mass., Waterbury, Ct., New York city and Newark, N. J. These places were all N.E. of the meteor and nearly in the same azimuth as New Haven. In the third column of the table below, is given the reported inclination, and in the fourth, the angle at which the path above assigned to the meteor would be seen to be inclined to the vertical circle at the horizon.

	Place	Observed inclin.	Calculated inclination	Obs. – cal.	Cal. – obs.	Observers.
1	Salem,	40°	31° 21′	8° 39′		Francis F. Wallis.
2	Waterbury,	30	34		4°	Wilder Smith.
3	New York,	10	35 27		25 27′	Mr. Gould.
4	" "	20	35 27		15 27	Mr. Latham.
5	" "	35	35 27		27	J. P. Pirrson.
6	" "	45	35 27	9 33		Mr. Bradley.
7	Newark,	45	36 33	8 27		Henry J. Mills.

The observations that appear to me from the circumstances of the observer most worthy of confidence next to that at New Haven are taken in order, the fifth, the second, and the seventh. These in fact agree best with the New Haven inclination. The third I feel disposed to leave out of consideration. Perhaps the fourth should be likewise discarded. The numbers in the fourth column, refer to the horizon, the third in general to points above it. They are not therefore exactly comparable. The numbers in the fifth and sixth columns appear large, but if we consider only those observations that appear worthy of confidence, I think the accordance is as great as can usually be expected for a daylight meteor. The probable error would be quite small. The errors are pretty nearly the same in excess as in deficiency. The assumed angle of inclination therefore cannot be far from the truth.

The inclination of the meteor's path as actually seen at Alexandria, must have been nearly the same as that resulting from the mean of the observations reported by Mr. Hallowell. The arc passing through the two given positions is at the horizon inclined 11° 34′ to the vertical. The meteor was seen by three persons at Washington, two of whom say it descended vertically.

The third says it apparently descended to the ground a little north of the point of its first appearance.

The computed trajectory would be seen at Washington inclined 9° 30′ to the vertical circle.

Mr. Nickerson, four miles west of Dover, says the column of smoke was nearly or quite vertical.

Mrs. Cowgill, six or seven miles west of Dover, says that if it varied at all from the vertical, it disappeared to the north of the point where it was first seen.

The inclinations at these two places of the computed trajectory, and the vertical, are 7° and 6½° respectively.

A line but little inclined to the vertical would be easily considered strictly vertical. But one would not readily mistake the direction of inclination. For my present purpose, moreover, an error of several degrees in the inclination at Alexandria would be of little importance.

The length of the visible track of the meteor is the next thing to be computed. Judge Boardman is confident that he saw it as far east as the line of Church street. It was probably visible sometime before he saw it. If S. 29° W. be assumed as the direction in which it was first seen we have a visible arc of about 12°, corresponding on the real line of the meteor to a distance of 36, or more exactly 35·91 miles. The motion was nearly at right angles to the line of vision. One extremity of this line was in N. lat. 39° 13′ 33″, W. long. 74° 24′ at an altitude of 49¼ miles. The other extremity was in N. lat. 39° 13′ 15″, and W. long. 74° 49′ 40″, at an altitude of about 22 miles. After the meteor disappeared to Judge Boardman, it must have passed several miles before it exploded and vanished. Many accounts agree in this, that it disappeared above the horizon. Some say it burst into fragments, each leaving behind a train of light. At Tuckerton, N. J., it disappeared at 10° altitude. Other accounts make me think this not far from the truth. This would make it explode at an altitude of a little less than 2°, as seen from New Haven. It would add 5° to the 12° of the visible arc and about 14 miles to the track of the meteor, making it in all, about 50 miles long from the point assumed, as that first seen by Judge Boardman.

Prof. Loomis, from the reports of several persons who saw the meteor in New York city, says, "the length of its visible path, was variously estimated from 15° to 25°. The entire period of its visibility did not exceed one or two seconds." By most of these observers, both the upper and lower portions of the meteor's path were probably not seen. At first it would not attract attention and it usually disappeared behind buildings. They probably saw in the average nearly the same amount of the meteor's path as Judge Boardman. This at New York city, subtends an arc of about 18°.

The meteor was no doubt seen at Alexandria much sooner than at New Haven. I am inclined to think that the reported altitude of its first appearance is less correct than the inclination. Taken strictly, the visible arc corresponds to a distance of 522 miles on the meteor's path. But a change of one degree in the place of first appearance might diminish this nearly 80 miles. When first seen at New Haven, it had an altitude of 17° 25′ at Alexandria. If the altitudes had been estimated, and not measured, the 17° 25′ might be considered 40°. But the method employed, that of directing an instrument to that point in the heavens, seems well fitted to give tolerably correct results. Though we cannot determine the distance passed over by the meteor while in view, yet it is evident that Mr. Martin saw it much sooner than Judge Boardman. If we were to allow an error of 15° in the place of first appearance, the length of the visible path of the meteor would be at the least 90·8 miles. If in addition, an error of 5° be allowed in the direction of the meteor's path, the length would not be less than 70 miles. If even the meteor's path was 10°, and the place of first appearance 15° in error, the visible path would be 57 miles.

These last errors seem very much greater than can be allowed. Mr. Hallowell says, "I cannot believe the body could possibly have had a less altitude than 35° at the time it was first seen." Ten degrees change in the direction of the meteor's path would carry it about 8° towards the vertical as seen from New Haven and New York city, a change which I think the observations would hardly allow.

The Washington observers, to some extent confirm the Alexandria observation. One says it appeared first at an elevation of 50°. Dr. Mackie says that the meteor had a luminous train extending vertically 15° to 20°. When first seen, its base was about 30° from the horizon. The point where it was first seen, by Judge Boardman, would have, at Washington, an altitude of 17° 40′.

The *time* of *flight* is the most difficult element to determine. My purpose is not to compute the actual velocity. I wish rather to prove that it was much more than 21 miles per second, when the body entered the atmosphere.

Judge Boardman estimates the time at one second, and says that it could not have been as great as two seconds. He estimated it, by supposing a body to pass with the same velocity over a similar distance, and noting the interval. The person to whom he was speaking, was not able to get a view of the meteor. The most probable velocity from this observation, would be 36 miles: the least velocity over 18 miles a second. The New York city observers, reported by Prof. Loomis, saw about the same amount of the meteor's path, as Judge Boardman. "The entire period of visibility did not exceed one or two seconds." The velocity then

would be not less than 18 miles a second, and probably it was much greater.

Mr. Mills estimated the time at two seconds. The arc passed over seems to have been 15° or 20°. This would give a velocity of about 18 miles a second, if his estimate of time is correct.

Dr. Mackie of Washington, says: "It was perhaps two seconds in view, for I had time, after seeing it first, to grasp my companion's arm, and point to it, before it disappeared." The altitude of its first appearance being estimated, and not measured, the velocity cannot be easily determined from this observation. But considering how liable an observer is to over-estimate the time of flight, I think the velocity, so far as indicated by his observation, is *much greater* than 20 miles a second.

The time of flight at Alexandria was estimated nearly in the same manner as at New Haven. The velocity which is indicated by this observation depends on the amount of error we can allow in the determination of the point of first appearance. Taken strictly, we have a velocity of 260 miles a second. Though I should not be unwilling to admit such a velocity, if we had valid proof of it, yet the present observation cannot be considered as furnishing it. Allowing a possible error as great as mentioned (p. 190) we have a velocity of 45, or 35, or 28½ miles per second. Only one person, that I am aware of, gives a period of time exceeding two seconds. Mr. Wallis of Salem, Mass., says it was in sight from five to eight seconds.

Besides these specific estimates of time, we have other reasons of greater or less weight for calling it very short.

Mr. Marsh in his paper argues with great reason, that "the extreme shortness of the time occupied in its flight is proved, not merely by the estimates of several observers, but by the failure of people in the vicinity of the explosion to distinguish the source of the sudden flash of light seen by them, and by the impression of even the most distant observers, that it fell very near to them." The latter reason, especially, has much weight.

The light is always called a "flash of light," by some a *sudden* or *instantaneous* flash.

A large number of observers state that they were unable to call the attention of those standing by them to the meteor. It seems that only those looking towards that part of the heavens, saw it.

In a letter dated June 13th, Mr. Marsh says, "all I have since heard from parties I have conversed with tends to confirm the shortest estimates, the impression generally being that it was instantaneous or nearly so."

In reasoning from these data, two considerations should be kept in mind.

1st. The natural tendency is to make the time of flight too great, and hence the velocity, too small.

2nd. From the moment the meteor entered the atmosphere, it would lose velocity. The resistance which the air offers to so rapid a motion, is enormous. If meteorites be admitted to come in general from meteors, it may be added that they rarely enter the ground more than two or three feet. They do not strike the earth with a velocity at all comparable to that which meteors are known to have, in the higher regions. They lose almost all their velocity in passing through the atmosphere.

A careful examination of all these observations leads me to believe that the actual velocity was as great as 36 miles a second. If we consider the resistance of the air, and then make as large an allowance for errors of observation as can reasonably be made, it seems almost impossible that it could have entered the atmosphere with a velocity less than twenty-one miles. The parts of the earth directly under the meteor, were by the earth's motion in its orbit, and on its axis, moving in a line inclined 89° 31′ to the path of the meteor, with the velocity of 19·023 miles. If the velocity of the meteor in this path was 21, its velocity relative to the sun would then be a little more than 28¼ miles. If the meteor had been moving in a parabolic orbit around the sun, it would have had from the combined action of the earth and sun, a velocity of 27·9 miles a second. If, therefore, as I think, can hardly be doubted, the meteor entered the atmosphere with a velocity not less than 21 miles, *it must have been moving in a hyperbolic orbit.*

We have been accustomed to consider the solar system as filled with small planetoids, millions of which, each day, come into the atmosphere, and are burnt up, causing the *shooting stars.* Now we find that we must, in all probability, add one, and no doubt innumerable other similar bodies to the stellar spaces. It opens a new view of creation.

It must not hence be imagined, that the meteors and shooting stars all come from the stellar spaces. The periodicity of the August and November meteors, shows plainly that they are from permanent members of the solar system.

This meteorite did not come from the moon. If we could suppose a lunar volcano to throw out a body with such an enormous velocity, that body must come to the earth, nearly from the direction of the moon. But the moon was at that time about 120° from the direction of the meteor's path.

The recent researches, respecting the transformation of motion into heat, throw some light on the subject of shooting stars. When these bodies come into the atmosphere, the motion they lose is transformed into motion of the air, heat, light, sound, and probably other forms of energy. If it was all transformed into heat, it would be easy to compute the amount due to the loss of a given velocity. If they have a motion of their own, and their directions are subject to no law, it is easily seen that the average

velocity is much greater than 19 miles a second. A body weighing one pound, and moving 25 miles a second, has momentum sufficient to raise $(25\times5280)^2\div2g=271{,}500{,}000$ pounds one foot. By Joule's equivalent the raising of 772 pounds one foot, corresponds to the heat necessary to raise one pound of water one degree Fahrenheit. If the capacity of the meteoric substance for heat is 0·2, (that of iron is 0·12,) the loss of a velocity of 25 miles would be equivalent to heating $(271{,}500{,}000\div0{\cdot}2)\div772=1{,}760{,}000$ pounds of the substance one degree Fahrenheit, if the whole of the motion was transformed into heat. A very small fraction of this heat would doubtless suffice to burn up, or dissipate, any substance whatever.

It is often urged, that the shooting stars cannot be solid bodies, since of the millions that daily enter the atmosphere, so few come to the ground. The above calculation shows that the heat generated may be ample to vaporize or dissipate them.

The shooting stars need not in general be large bodies. The apparent size is due to irradiation, and indicates, not amount of matter, but rather amount and intensity of light. Thus the stars though often spoken of as mere points have disks. The diameter of stars of the first magnitude was estimated at 2′ by Tycho Brahe. The telescope has shown that this disk is spurious. If these stars are equal in size to the sun, Tycho's estimate makes their diameters 50,000 times too great.

It has been estimated that the light of the sun's surface is four or five times as great as that of the same surface of the lime in the calcium light. It is also estimated that the light of the sun is 20,000,000,000 times that of Sirius. A simple calculation shows that an inch globe as brilliant as the calcium light, would give at over 100 miles distance as brilliant a light as a star of the first magnitude. The estimates which are used as the basis of calculation are confessedly very vague, yet they show that a very small body may furnish as much light as a shooting star. Such a body would naturally burn up without passing through the atmosphere.

I can therefore see no reason, as some persons do, to make a marked distinction between the different classes of meteors. Those which furnish meteorites, those which explode with a loud report, and those of all degrees of brilliancy which are not heard to explode, all seem to belong to one class, and to differ from each other no more than substances on the earth. That some are solid and others aëriform is not impossible. Differences of chemical constitution, size, velocity, and orbit exist, and these may account for the variety of appearance.

Note.—Since the above was in type, the meteor of July 20 seems to furnish better data for proving that meteors sometimes come from the stellar spaces.

ART. XVIII.—*Crystalline form not necessarily an indication of definite Chemical Composition: or, on the possible variation of constitution in a mineral species independent of the Phenomena of Isomorphism.* By JOSIAH P. COOKE, Jr., A.A.S., Professor of Chemistry and Mineralogy in Harvard College.*

IN a memoir presented to the American Academy of Arts and Sciences in September, 1855,† I described two new compounds of zinc and antimony which I named *stibiobizincyle* and *stibiotrizincyle*, on account of their analogy in composition to the metallic radicals of organic chemistry. The symbols of these compounds are $SbZn^2$ and $SbZn^3$; and they are distinguished by the high perfection of their crystalline forms, the last being still further characterized by a most remarkable property of decomposing water quite rapidly at 100° C. I stated in the same memoir that crystals of these two compounds could be obtained containing proportions of zinc and antimony differing very widely from those required by the law of definite proportions; and I also traced out the relation between the composition of the crystals, and that of the menstruum in which they are formed. It is my object in the present paper to consider the bearing of these facts, already fully described, on the idea of mineral species, and to offer a few suggestions which I hope may be of service in determining the true chemical formulæ of many minerals, and thus in simplifying the science of mineralogy. But in order to render myself intelligible, it will be necessary to recapitulate very briefly the facts in question, referring to the original memoir for the full details.

The crystals both $SbZn^2$ and $SbZn^3$ can be obtained with great readiness. It is only necessary to melt together the two metals in the atomic proportions, and when the metals are fully alloyed, to proceed exactly as in crystallizing sulphur. The melted mass is allowed to cool until a crust forms on the surface, which then is broken, and the liquid metal remaining in the interior poured out. On subsequently breaking the crucible, the interior is found lined with magnificent metallic crystals, which, when not tarnished by oxydation have a silver-white lustre. In the course of my investigations on these compounds, crystallizations were made, or attempted, of alloys, differing in composition by one half to five per cent, according to circumstances, from the alloy containing 95 per cent of zinc, to that containing 95 per cent of antimony; *but only two crystalline forms were observed*, that of $SbZn^2$ and that of $SbZn^3$. The crystals of the

* Communicated by the Author.

† Transactions of the American Academy of Arts and Sciences, New Series, vol. v, p. 337. This Jour. [2], xx, 222.

two compounds both belong to the trimetric system; but they differ from each other, not only in their crystallographic elements, but also in their whole "habitus." Stibiotrizincyle crystallizes in long acicular prisms, which group themselves together into larger prismatic aggregates; while stibiobizincyle crystallizes in broad plates, which twin together on an octahedral face, and form a very characteristic cellular structure. This very striking difference in the character of the crystals proved to be an important circumstance in the investigation, as it enabled me to distinguish with certainty between the two compounds, even when the faces of the crystals were so imperfect that a measurement of angles was impossible.

The most remarkable result of the investigation, and the one to which I wish to direct especial attention, is the fact that each of the two crystalline forms was found to be constant under very wide variations in the per-centage composition of the crystals. As this is a point of great importance, it will be necessary to enter more into detail, considering in the first place the crystals of $Sb\,Zn^3$. The crystals of this compound are obtained in the greatest perfection from an alloy containing the two metals in just the proportions represented by the formula, namely, 42·8 parts of zinc, and 57·2 parts of antimony. They are then comparatively large, generally aggregated, and, as the three analyses cited in the accompanying Table indicate, they have the same composition as the alloy.

Composition of the alloy by synthesis.		Composition of the crystals by analysis.		
Per cent of Zn.	Per cent of Sb.	Per cent of Zn.	Per cent of Sb.	Sum.
42·80	58·20	43·15	56·93	100·08
"	"	43·06	56·50	99·56
"	"	42·83	57·24	100·07

On increasing gradually the amount of zinc in the alloy up to 48·7, the crystals continued to have the composition of the alloy; and the only difference which could be observed in their character was that they were smaller, and more frequently isolated. Between these limits the whole mass of the alloy exhibited a strong tendency to crystallization; and by pouring it, as it cooled, from one vessel to another, it could be crystallized to the last drop. On increasing the amount of zinc in the alloy to 50·7 per cent, the amount of zinc found in the crystals was uniformly less than it was in the alloy; but no closer relation between the two could be detected, owing, undoubtedly, to the unavoidable irregularity in the crystallization of the alloys which contained more than 50 per cent of zinc. This arose from a peculiar pasty condition which the liquid mass assumed at the point of crystallization. Definite crystals, however, were obtained from an alloy of 60 per cent zinc containing 55 per cent; above this the crys-

tals became less and less abundant, and gradually faded out, although the alloy of 86 per cent of zinc exhibited a radiated crystalline texture; and a trace of this structure could still be discovered even in the alloy containing only 4 per cent of antimony. It was very interesting to trace the gradual fading out of the crystalline structure, as the character of the phenomenon was entirely analogous to that which may be noticed in many crystalline rocks.

Finding that the crystalline form of $Sb\,Zn^3$ was constant under so great an increase of the proportion of zinc in the crystals, it might be supposed that, on returning to the alloy of 42·8 per cent of zinc and increasing the amount of antimony, we should obtain crystals containing an excess of antimony; but so far is this from being true, that the slightest excess of antimony entirely changes the character of the crystallization. On crystallizing an alloy containing 41·8 per cent of zinc, not a trace of any prismatic crystals could be seen; but in their place there was found a confused mass of thin metallic scales, which, as will soon be shown, are imperfect crystals of $Sb\,Zn^2$. Thus it appears that, although perfectly formed crystals of $Sb\,Zn^3$ can be obtained containing 55 per cent of zinc (that is, 12 per cent above the typical proportions), they cannot be made to take up the slightest excess of antimony.

Let us pass now to the crystals of $Sb\,Zn^2$. In order to obtain crystals having the exact typical constitution, it was found necessary to crystallize an alloy at least as low as 31·5 per cent of zinc. At this point large compound crystals are obtained corresponding to the large crystals of $Sb\,Zn^3$; and the same was true of alloys down to 27 per cent of zinc. Between these two limits (namely, alloys of 31·5 and 27 per cent of zinc) the crystals formed were found to have the theoretical composition of $Sb\,Zn^2$, indicating of course a tendency towards this point; but on increasing or diminishing the amount of zinc in the alloy beyond these limits, the composition of the crystals immediately began to vary in the same direction as that of the alloy. The crystals of $Sb\,Zn^2$ containing an excess of zinc are smaller and more frequently isolated than those having the exact theoretical composition. A similar fact, it will be remembered, is true of the crystals of $Sb\,Zn^3$.

At the alloy of 33 per cent of zinc, the definite crystals of $Sb\,Zn^2$ begin to disappear, and are succeeded by thin metallic scales, which are obviously imperfect crystals of the same form. This was established, not only by the obvious law of continuity noticed in the different specimens (the perfect crystals gradually passing into the scales), but also by the peculiar mode of twining, which was the same with the scales as with the large crystals, forming the peculiar cellular structure already referred to. Moreover, the angle between two scales thus united was found

to be equal to the basal angle of the perfect crystals, at least as nearly as could be measured. These scales continue up to the alloy of 41·8 per cent of zinc, becoming, however, less abundant and less distinct. Several specimens of them were analyzed; but no regularity could be detected in their composition, except that they all contained a much larger amount of zinc than the alloys in which they were formed.

Crystals of $SbZn^2$ containing an excess of antimony were readily obtained from alloys containing less than 27 per cent of zinc. They became more and more imperfect as the excess of antimony increased, and finally faded out altogether in the alloys below 20 per cent of zinc. It is evident, therefore, that definite and perfect crystals of $SbZn^2$ can be obtained with a large excess either of zinc or antimony above the theoretical composition. It is also evident that, of the two compounds, $SbZn^2$ is the most stable,—first, because it is formed to the exclusion of $SbZn^3$ in all alloys containing less zinc than the amount corresponding to the typical composition of the last compound; and secondly, because the crystals retain the typical composition under quite a wide variation (viz. between 31·5 and 27 per cent) in the composition of the alloy.

The facts above stated are fully illustrated by the following Table, which gives the results of a large number of analyses of crystals of both compounds formed in alloys containing different proportions of the two metals:—

Analyses of the Crystals formed in the Alloys of Zinc and Antimony.

Stibiotrizincyle.					Stibiobizincyle.				
Composition of the alloys by synthesis.		Composition of the crystals by analysis.			Composition of the alloys by synthesis.		Composition of the crystals by analysis.		
Per cent of Zn.	Per cent of Sb.	Per cent of Zn.	Per cent of Sb	Sum.	Per cent. of Zn.	Per cent of Sb.	Per cent of Zn.	Per cent of Sb.	Sum.
70·40	29 60	64·15	35·77	99·92	33·00	67·00	35·37	64·57	99·94
66 50	33·50	61·00	39·00	*100 00	33 00	67·00	35·40	64·60	†100·00
64 50	35·50	53·50	41·44	99·94	32·50	67·50	34·62	64·92	99 54
....	....	55·49	44·42	99 91	32·50	67·50	34·61	65·39	†100·00
60·60	39·40	55·00	45 09	100·09	31·50	68·50	33·95	66 09	100·04
58·60	41·40	50 39	49·29	99·68	29·50	70·50	33 62	66·38	†100·00
56·60	43·40	49·92	50 05	99·97	29·50	70·50	33·62	66·38	†100 00
54·70	45·30	48·26	51 42	99 68	27·50	72·50	33·85	65·81	99 66
52·70	47·30	47·47	52·53	†100·00	26·50	73·50	32·08	67·60	99 68
....	....	....	....	....	26·00	74·00	30·74	69·06	99·80
50·70	49·30	46·89	53·11	†100·00	25·50	74·50	30·43	69 51	99·94
50·70	49·30	46·45	53 55	†100·00	25·00	75·00	29·88	70·20	100 08
48·70	51·30	48 66	51·34	†100·00	24·50	75·50	28·76	71·24	100·00
46·70	53·30	46·77	53·23	†100·00	23·50	76·50	27·93	71·85	99·78
44·80	55·20	44·26	55·73	†100·00	22·50	77·50	26·62	73·27	99 89
43·80	56·20	44·04	55 96	†100 00	21·50	78·50	24·83	74·74	99·57
42·80	58·20	43·15	56 93	100 08	20·12	79·88	20·58	79·42	100·00
42·80	58·20	43 06	56·50	99·56					
42·80	58·20	42·83	57·24	100 07					

* In this analysis the antimony only was determined.
† In this analysis the zinc only was determined.

The relation between the composition of the crystals $SbZn^2$ and that of the alloy in which they are formed, is discussed at length in the memoir already referred to. It is there shown to be a very simple function of the mass of metal which is in excess in the alloy, and of the force which determines the union of the elements in definite proportions. The whole order of these phenomena seem to the Author to point to the existence of a power in the mass of metal which is in excess in the alloy, to disturb the action of the force, whatever it may be, which tends to unite the elements in definite proportions. There is, in the first place, a strong tendency in the elements to unite and form crystals having the exact typical composition; and secondly, this tendency is only overcome by a certain excess of either metal in the alloy. Then, again, the crystals of one compound obviously interfere with those of the other. This certainly has the appearance of one force interfering with the action of another,—the force of mass (if I may so call it) perturbing the action of the chemical force. But it is not my object at present to enter into a discussion on the cause of this variation. Moreover, since such a discussion must be based on purely hypothetical grounds, we could not expect to arrive at any definite conclusion. The facts will be viewed differently according to the theory which may be adopted in regard to that long-controverted subject, the essential constitution of matter. Leaving, however, all theoretical considerations aside, there are certain practical bearings of the *observed facts* on the science of mineralogy which are of immediate application.

Here are two beautifully crystallized products, as well crystallized as any that occur in nature, and yet the different specimens of the crystals differ from each other so widely in composition that any single analysis might lead to an entirely erroneous conclusion in regard to the general formula of the substance. Were a chemist to analyse accidentally solely the crystals obtained from an alloy containing 58·6 per cent of zinc, he would at once determine that the formula of the compound was $SbZn^4$; and by a like accident he might be led to any other formula between this and $SbZn^3$: in fact, by an analysis of a number of specimens of needle-shaped crystals obtained from alloys of copper and tin, Rieffel was led to several just such improbable formulæ; and in my own investigations it was not until I had analysed a whole series of crystals, that the real nature of the phenomena became apparent, and the true constitution of the compounds determined. If, then, such great variations in composition are compatible with a definite crystalline form in these furnace products, may not similar variations occur in the crystalline minerals formed in nature?

It is not necessary to make an extended investigation in order

to answer this question; for the materials at our hands are sufficient to give us a satisfactory reply.

There is a compound of antimony and silver called discrasite, which occurs in many localities crystallized in trimetric prisms homœomorphous with $Sb\,Zn^3$. The formula of the mineral is therefore probably $Sb\,Ag^3$, which would require 71·5 per cent. of silver; but the per cent as given by analysis varies between 75·25 and 78 per cent, and one analysis gives the per cent as high as 85. Further analyses of this mineral are required in order to determine its constitution, but there can be no doubt that it varies in composition like $Sb\,Zn^3$.

Silver-glance is another highly crystalline mineral. Theoretically it should contain 87·1 per cent of silver and 12·9 per cent of sulphur; but in a specimen analysed by Klaproth, the proportions were 85 and 15.

Again, the analyses of pyrrhotine (magnetic pyrites) give results varying between 38·78 per cent sulphur, 60·52 per cent iron (variety from Bodenmais), and 43·63 sulphur, 56·37 iron (variety from Barèges). The constitution of the mineral is still uncertain; but its true formula is probably Fe S, which would require 36·4 per cent sulphur and 63·6 per cent iron. Lastly, the analyses of antimony-glance give results varying between

Antimony 74·06,	and	Antimony 73·5,
Sulphur 25·94,		Sulphur 26·5.

The true formula of this mineral is undoubtedly $Sb\,S^3$, which would require only 72·88 per cent of antimony.

Similar examples might be greatly multiplied. Those just cited were selected at random from the first few pages of Dana's 'System of Mineralogy.' They are all examples of binary compounds which occur almost chemically pure in nature; so that the phenomena in question are not complicated by those of isomorphism.

When we pass to minerals of more complex constitution, the same phenomena can be made evident, although not quite so easily, on account of the introduction of the phenomena of substitution by isomorphous elements. It will not, however, be necessary for me to cite examples; for it is a fact perfectly well known to all mineralogists, that, after making allowances for the substitution of isomorphous elements, the various analyses of such minerals as mica, hornblende, garnet, and tourmaline differ very greatly from each other,—a difference, moreover, which no mere error of analysis will explain, and which must therefore be referred to an actual variation in composition. In the silicates this variation in composition is made evident by the variation of what is termed the "oxygen ratios;" and it is well known to mineralogists that in many species this variation is very large.

For example, in mica the following ratios between the oxygen in the base and acid have been observed in merely the Muscovite variety:—13 : 16, $13\frac{1}{3}$: 16, and $14\frac{2}{3}$: 16; and similarly wide variations might be pointed out in other well known species. It is in consequence of such variations as these that the general chemical formulæ of some of the best known mineral species, such as mica and tourmaline, are still uncertain; and in other cases, where the true formulæ is probably known, the constitution of the mineral has been determined quite as much from other considerations as from the chemical analyses.

Sufficient has been said, I think, to show that variations in composition similar to those which I have observed in zinc and antimony occur in many minerals; and I trust that the results of my investigation will serve to throw light on this whole class of phenomena, which have so greatly perplexed mineralogists, and rendered all strictly chemical classifications of mineral species so unsatisfactory. This investigation has shown that a definite crystalline form is compatible with quite a wide variation of composition, and has in this way pointed out an explanation of the variation observed in the mineral kingdom. But more than this, the investigation has also indicated a method by which, amidst all this variation, the true constitution of the mineral can be determined.

In the compounds of zinc and antimony, although the definite crystalline form was compatible with a wide variation in the proportions of the constituent elements, yet the point corresponding to the typical composition was marked by several unmistakeable properties, which clearly enough indicated the true formulæ of the compounds. These properties are discussed at length in my original memoir, and need therefore only to be alluded to in this connection.

It has already been stated that the crystals, both of $Sb\,Zn^3$ and $Sb\,Zn^2$, having the theoretical composition are, as a rule, larger and more generally aggregated than those containing an excess of either metal. Moreover, in $Sb\,Zn^2$ the general character of the crystals appears to be modified by the change of composition, although the crystallographic elements remain the same. Thus in the crystals having the theoretical composition, the octahedral planes are greatly developed, giving to the crystals the general appearance of a truncated octahedron.* But as the crystals take up an excess either of antimony or zinc, the basal planes become more and more dominant, and the crystals are at last reduced to thin plates. In fact, so marked are these changes, that, after a little experience, a person could tell the approximate composition of the crystals from their general appear-

* See figure accompanying my original memoir.

ance. Similar changes in the appearance of many minerals are familiar to the mineralogist. They are seen in calcite, heavy spar, Anglesite, and others, and may serve as guides in tracing variations of composition.

Again, the specific gravity of the crystals, both of $Sb\,Zn^2$ and $Sb\,Zn^3$, was taken with great care through the whole series, and the results are tabulated below. The union of the two elements is attended with an increase of volume, and this increase is at a maximum at the points corresponding to the theoretical composition. These points would therefore be marked in a set of crystals by being points of minimum specific gravity; and they could be determined with great accuracy by means of this property, even in a series of alloys of the two metals which had not been crystallized. This fact is illustrated by the following Table, reprinted from the original memoir.

Specific Gravities of Crystals formed in the Alloys of Zinc and Antimony.

Composition of the alloys.		Composition of the crystals.		Spec. grav. of crystals by experiment.	Mean spec. grav. of zinc and antimony.	Expansion in crystallizing.
Per cent of Zn.	Per cent of Sb.	Per cent of Zn.	Per cent of Sb.			
10 00	.....	....	....	7·153	7·153	0·000
*96 00	4·00	....	....	7·069	7·133	0·064
*86 20	13·80	....	....	6 898	7·082	0·184
*76·30	23·70	....	....	6·769	7·032	0 263
70·40	29 60	64·20	35·80	6 699	6 975	0·276
66 50	33 50	61·00	39 00	6 628	6·959	0·331
64 50	35 50	58·56	41·44	6·596	6·948	0 352
62 50	37 50	55·53	44·47	6·506	6·933	0·427
60·60	39·40	55 00	45 00	6·440	6·931	0·491
58 60	41·40	50 39	49·61	6·396	6 909	0·513
56·60	43·40	49·95	50·05	6·388	6·906	0·518
48·70	51 30	48·66	51·34	6·404	6·900	0·496
46·70	53·30	46 77	53·23	6·376	6·891	0·515
44·80	55·20	44 26	55·74	6 341	6 879	0·538
†42·80	57·20	43·09	56·91	6·327	6·874	0·547
*40·00	60·00	....	....	6 386	6·860	0 474
*35 00	65·00	....	....	6 404	6·837	0·433
33 00	67·00	35 37	64·63	6·401	6 838	0·437
‡29·50	70·50	33·62	66·38	6·384	6 830	0·446
‡27·50	72·50	33·85	66·15	6·383	6 831	0· 48
26·50	73 50	32 08	67·92	6·400	6 822	0·422
26·00	74 00	31·07	68·93	6·418	6·818	0·400
25·50	74·50	30·43	69 57	6 428	6 816	0·388
24·50	75·50	28·76	71·24	6·449	6 807	0·358
22·50	77·50	26·62	73 38	6·453	6·798	0 345
21·50	78·50	24·83	75·17	6·467	6·790	0·323
*15 00	85 00	....	....	5 564	6·744	0·180
*10 00	90 00	....	....	6 603	6·721	0 118
*5 00	95 00	....	....	6·655	6 698	0 043
....	100·00	....	....	6 677	6·677	0 000

* Alloys not crystallized. † Point of typical composition of $Sb\,Zn^3$.
‡ Point of typical composition of $Sb\,Zn^2$.

The point of typical composition in the case of the crystals of $SbZn^3$ was still further marked in a most decided manner by a very remarkable property. It has already been stated that this compound has the power of decomposing water with rapidity at 100° C.; but this is true only of those crystals which have approximately the theoretical composition. During the course of my investigation I determined the quantity of hydrogen evolved by alloys of different composition during a given time, taking care, of course, that the circumstances should be the same in all cases; and I found that with the alloy containing 43 per cent of zinc, there is an immense maximum, confined at most between 2 per cent on either side, the alloy of 43 per cent yielding over nine times as much gas as an alloy of 50 per cent, although the crystals of the last were fully as definite as those of the first.*

It is evident from the above facts, that the points corresponding to the theoretical composition of the two compounds of zinc and antimony, are also *points of maxima and minima* of various properties. Now I have no doubt that the same truth will be found to hold in the mineral kingdom. In a mineral like tourmaline or mica, for example, the specimen having the exact theoretical composition may probably be discovered by examining a large number of specimens, and discussing their various physical properties. All the physical properties may be of value in this connection, such as lustre, hardness, specific gravity, specific heat, &c.; and no mechanical rules can be laid down. Much must depend on the discretion of the observer; and in any cases such properties will be selected as are best adapted to the circumstances of the case. In comparing different crystals of the same mineral, it is obviously important to select such as have been formed in a different matrix; for it is only with such that we should be led to expect great variations of composition. It is also evident that the phenomena would be complicated when there has been a substitution of isomorphous elements; and until the effect of such substitution on the physical properties can be traced, it will be necessary to select specimens of as uniform a constitution as possible.

With one other consideration I will close this paper. The principle which has been here discussed must modify materially our notion of a mineral species. The idea of a mineral species has hitherto involved chiefly two distinct characters:—first, a definite crystalline form; second, a constant general formula; and any important variation in either of these characters has been regarded as equivalent to a change of species. Rutile and anatase are regarded as different species, because their crystalline forms are slightly different, although both minerals have identically the same constitution; and again, magnetite and Franklin-

* See Table in the memoir before cited.

ite, which have the same form, are regarded as different species, because they have a slightly different composition. It is true that the actual composition of a mineral may vary very greatly by the substitution of isomorphous elements, and yet, if the general formula remains constant, the species may not be changed. But the extent to which such substitution can be carried without changing the species is not so well settled among mineralogists as could be desired, and the same rule is not applied to all species. The difference between the *varieties* of garnet, for example, is as great as that between the *species* magnetite and Franklinite. Leaving, however, this point undetermined, all mineralogists have agreed that any *essential change* in the *general formula* was inconsistent with the idea of the *same species*. The result, however, of my investigation is to show that the general formula of a mineral species may vary also, or, as I should rather say, the *general formula* is not necessarily the actual formula of each given specimen, but only the *typical formula* of the species towards which the mineral tends, and which it would unquestionably reach if it could be several times recrystallized.

According to this view, the general formula represents not the *actual constitution* of the mineral, but only a certain *typical composition*, which perhaps is never realized with any actual specimen. The fact that the composition of a mineral species may be modified by the substitution of isomorphous elements, was first established by Mitscherlich, and has long been an admitted principle in mineralogy. We must now, as I think, still further expand our idea of a mineral species, and admit that its composition may be modified by an actual variation in the proportions of its constituents. Thus it is that in mineralogy, as in other sciences, we are led to admit the truth of that maxim which every advance in true knowledge seems to verify, "Natura non facit saltus."

While the results of my investigations thus serve to render the idea of a mineral species less definite than before, I cannot but hope that they will tend ultimately to simplify the whole subject of mineralogy; for not only may we expect to reduce the number of mineral species, but also, by simplifying the general formulæ of those which remain, to classify the whole with a greater precision than is now possible. To do this, however, implies a careful revision of the whole subject-matter of mineralogy on the principles above given,—a labor of which few can appreciate the extent, except those who are familiar with the methods of physical research. The work cannot be done by any one person; and it is the chief object of the present paper to call the attention of mineralogists to the importance of the subject.

I have not thought it necessary to dwell in this paper on the obvious distinction between the phenomena here in considera-

tion, and those of isomorphism. It was shown in my previous memoir, that the variation in the composition of the crystals of $SbZn^3$ and $SbZn^2$ could not be explained by this principle; and the distinction between the two classes of phenomena has been still further illustrated by a recent investigation on the crystals formed in alloys of copper and zinc, made in my laboratory by Mr. F. H. Storer. These crystals, which are undoubtedly mixtures of isomorphous elements, give no indications whatever of points of typical composition,—thus illustrating not only the characters of an isomorphous mixture, but also the distinction between such a mixture and a true chemical compound. Admitting, then, the possibility of a variation of composition in a mineral species, independent of the phenomena of isomorphism, it becomes of importance to distinguish this new class of phenomena by a separate term; and I would propose for this purpose the word *Allomerism*. By this word I would designate *a variation in the proportions of the constituents of a crystallized compound without any essential change in the crystalline form*. If, then, we also use the word *typical* to indicate the condition of *definite composition*, we may speak of those specimens of a mineral species which contain an excess of one or the other constituent, as *allomeric variations from the typical composition*. The degree of allomerism would then be measured by the excess of the *allomeric constituent* above the typical composition. Thus the crystals of $SbZn^3$ containing 42·3 per cent of zinc would be said to have the typical composition; while those containing 55 per cent of zinc would be distinguished as an allomeric variety, the degree of allomerism in this instance amounting to 12 per cent, and zinc being the allomeric constituent. In the case of the mineral Discrasite, it is probable that no specimen having the typical composition has yet been analyzed. Those specimens whose analyses are given in Dana's 'System of Mineralogy,' are all probably *allomeric varieties* of the mineral, silver being the allomeric constituent, and the degree of allomerism varying from 4 to 7 per cent. It is unnecessary, however, to multiply examples, as the above are sufficient to illustrate the use of the term.

ART. XIX.—*Notices of several American Meteorites;* by CHARLES UPHAM SHEPARD.

1. *Nebraska Iron.*—This very interesting mass, first noticed in a late number of the Proceedings of the St. Louis Academy of Sciences, was brought to St. Louis by the fur traders in the employ of C. P. Chouteau, Esq., about two years ago, and by him presented to the museum of the Academy. It was found near the Missouri River, between Council Bluff and Fort Union. It

originally weighed about thirty-five pounds, but is now reduced to twenty-nine. Its shape was an oblong, compressed oval, not unlike that of the Chesterville, South Carolina, iron-mass, which has been compared by me to the form of a thick, blunt edged fresh water clam (*Unio*). Its surface is as black and smooth as that of the Braunau Iron, from which however it differs in being more even and smooth, though it is not destitute of the usual indentations belonging to meteorites, but these are by no means uniform in their occurrence over the entire surface. The crust is everywhere extremely thin, amounting to scarcely more than a mere varnish; and what is very remarkable, is often insufficient to hide the Widmannstättian figures with which the body must have been covered before it entered our atmosphere. The lines are not equally displayed throughout, and indeed will generally require a single lens in order to be distinctly seen. Nor have they the same beautiful regularity as when obtained by etching upon a polished surface from the interior. They are moreover curiously knotted, so as to resemble, under the microscope, the blunted teeth of a fine saw blade. The configuration upon the etched plates of this iron resembles slightly that of the Texas mass, though the bars are much more rectilinear, and in this respect approach nearer to the new African irons described by me (those from Namaqua Land and Orange River). I observe, however, in this as well as in most other irons, that the fullest regularity of internal structure does not prevail, until some little depth from the outer surface or crust is reached. The Nebraska iron is quite free from earthy, plumbaginous or pyritic matter. Prof. Litton, of the St. Louis University, has recognized in it the presence of nickel; and is understood to be now engaged with its complete analysis. Its specific gravity is 7·735. The character of the surface renders it certain that this mass must be of very recent fall. I am indebted to the liberality of the Academy, and the kind offices of Nathaniel Holmes, Esq., its Secretary, for a fine slice of the mass, from which I have been enabled to make the foregoing observations.

2. *Forsyth* (Taney County, Missouri) *Iron.*—My first information of this locality was derived, while on a visit to southeastern Missouri in April last, from N. Aubushon, Esq., of Ironton. He stated that a small specimen of curiously knitted, malleable ore, of a white color resembling silver, had been sent him two or three years ago by a person residing near the locality. Mr. Aubushon forwarded it to an assayer at Ducktown, Tenn., from whom he learned that it was composed of iron and nickel. On visiting St. Louis soon after, I was informed by Prof. Swallow, the State Geologist, that a specimen had also been transmitted to him by letter from the same place; and that Prof. Litton had found it to be composed of similar constituents. Prof. Swallow

presented me a small fragment of his specimen, upon which I am able to offer a few remarks, awaiting the results of Prof. Litton's analysis, for fuller information.

The mass evidently belongs to the rather rare group of amygdaloidal meteoric irons, in which, like those of Steinbach (Saxony) and Hainholz (Westphalia), the peridotic ingredient preponderates over the nickelic iron. Its specific gravity is 4·46. The iron is remarkable for its whiteness, while the peridot is of a well marked green color, and distinctly crystalline. No pyrites is visible in the very small fragments examined. It is reported that two considerable masses of this meteorite were found buried in the soil upon a hill-side; and that they are at present secreted under the belief that they contain silver.

3. *Bethlehem* (New York) *Meteoric Stone*, of Aug. 11, 1859.—The only stone found from the great explosion heard over a large district of northwestern Massachusetts, and extending into the state of New York as far as ten miles west of the cities of Albany and Troy, was the little fragment, less in size than a pigeon's egg, of which an outline is here subjoined.

I am imdebted to David A. Wells, Esq., the editor of the American Scientific Annual, for several interesting particulars concerning its discovery and properties. He was good enough to visit, at my request, the residence of Mr. Garritt Vanderpool (situated seven miles from Albany and one mile west of Bethehem church), where the stone fell, and to ascertain on the spot the facts respecting its descent. Mr. Vanderpool was at work near his house, and heard the explosion in common with other members of his family. About two minutes after, as it appeared to him, a stone, coming in an oblique course, struck the side of a waggon-house, glanced off, hit a log upon the ground, bounded again, and rolled into the grass. A dog lying in the doorway of the waggon-house sprang up, darted out and seized it, but dropped it immediately, probably on account of its warmth and sulphureous smell. Mr. Wells had two opportunities of inspecting the stone before it was sold to the State Cabinet in Albany. It was far from being entire when first picked up, no doubt having been broken by its contact with the house. On the second inspection, he noticed that one corner had been broken away, and other portions much marred through the use of knife blades upon its surface by the curious, who, in this rude way, had been led to investigate its peculiarities. About "one-half of it however," he observes, "is covered with the peculiar dark colored crust of meteorites, and has a burnt appearance. This is so well marked that it at once establishes its identity as a meteoric stone. The other sides present-

ing the appearance originally bright and of a fresh fracture were clear, but are now soiled from handling. The color is a light steel-grey, with metallic particles interspersed. The structure is granular."

Through the recommendation of His Excellency, Gov. Morgan, to the officers having in charge the state cabinet, a small fragment of the stone including a portion of the crust, was most obligingly transmitted to me by Mr. Woolworth, accompanied by the following note:

"Albany, Nov. 11, 1859.

Prof. CHARLES U. SHEPARD,

Dear Sir:—I am directed by Gov. Morgan, as Chairman of the Committee of the Regents of the University on the State Cabinet of Natural History, to send you the inclosed portion of the aërolite lately found near this city. The Committee had hopes of finding other parts of the stone than the one first discovered, but have not been successful. They regret they cannot send you more, but could not do so without destroying the specimen they possess. Hoping it may be sufficient for your purposes, I am, very truly, yours, &c.,

J. B. WOOLWORTH, *Sec'ry, &c.*

I am likewise much indebted to Henry A. Homes, Esq, the State Librarian, for his good offices in facilitating my early acquisition of the specimen which enables me to compare it with those I possess from other localities.

The crust of the Bethlehem stone is very peculiar. It is double the thickness of any in my collection, equalling that of thick pasteboard. It is perfectly black, and very open in its texture. The outer surface is rough, being nowhere perfectly fused, but only semi-vitrified. Without being fragile or carbonaceous, it nevertheless resembles in color, lustre, and porousness, certain surfaces of mineral charcoal. The interior of the stone is equally peculiar, being loosely granular, the particles being uniform in character, small, highly crystalline, and nearly transparent. They possess a brilliant lustre, a very light grey or greenish white color. They resemble volcanic peridot more than any species of the augitic or feldspar family. Nickelic iron, of a bright white color, in delicate filaments and semi-crystalline grains, is thickly diffused through the mass; and these grains, as well as those of the peridotic mineral, are flecked with brilliant points of pyrrhotine (FeS). The specific gravity is 3·56. In general color and effect to the eye, it approaches nearest to the Klein-Wenden stone (Sept. 16, 1843); but it differs from this in being larger grained, and looser in its texture.

4. *Remarks upon the Ohio stones of May* 1, 1860.—Through the much valued assistance of Prof. J. L. Smith, the large 53-pound stone that fell near the house of Mr. Wm. Law of New Concord, forms part of my meteoric cabinet. Without attempting at present a complete description of its form and character,

I will only offer a few remarks upon the relationship of the Ohio meteorites to those of other falls. In its internal aspect it approaches the stone of Jekaterinoslaw, Russia (1825), though it is somewhat firmer and more compact. In crust, the two are identical. It is also similar to the stone of Slobodka, Russia (Aug. 10, 1808); and compares closely with those of Politz (Oct. 13, 1819), of Nanjemoy, Maryland (Feb. 10, 1828), and of Kuleschowka, Russia (March 12, 1811); but the crust is less smooth on the Ohio stone than in that of the latter.

A pearl grey peridot forms the chief constituent (above two-thirds) of the stone. This mineral is often rolled up into obscurely formed globules, which are so firmly imbedded in the more massive portions of the same mineral, as to be broken across on the fracture of the stone, which thereby presents a sub-pisiform appearance. Snow white particles of Chladnite are thickly scattered in mere specks through the mass, and closely incorporated with the peridot. The nickelic iron, of a bright white color, is also everywhere thickly interspersed in little points. Pyrrhotine is less conspicuous, though often visible in rather broad patches; while black grains of chromite are easily distinguishable by the aid of a glass, and sometimes with the naked eye.

The crust is of medium thickness, and the usual wavy and pitted impressions are also strictly characteristic of these stones. Their origin in meteorites generally, is perhaps still obscure, but may be conceived to originate in the flaking off of fragments in consequence of the sudden transition from cold to hot, which must happen to bodies coming instantaneously from a temperature far below zero into a state of vivid incandescence, at least upon their immediate surface. We see a somewhat analogous flaking up from heated surfaces of granite blocks during a conflagration, when wetted by cold water; though in the latter case, as might be expected, convexities take the place of concavities.

5. *Supposed Fall of a Meteoric Stone in Independence County, Iowa, during the summer of* 1857.—I casually learned while recently in Missouri, that a stone fell at a place called Pilot Grove, near the stage road, in or near the month of August, 1857. The stone was preserved; and I am not without hopes of obtaining a portion of it, having heard of its exhibition during the last year before the Academy of Sciences at Chicago.*

* *Detection of Phosphorus in the native steel of Montgomery (Vermont), and in the Waterloo (New York) Meteoric stone:*

I have examined the first named substance chiefly with a view to determine its relation-hip to the Rutherfordton (N. C.) Ferrosilicine, (see the September number of this Journal for 1859), and find that while it is free from silicon, it nevertheless abounds in phosphorus. The Waterloo stone, whose resemblance is so great to a well burnt Bristol brick, gives a very decided test for phosphoric acid. The problematical steel from Bedford County (Pennsylvania) is free both from silicon and phosphorus.

New Haven, July 1, 1860.

ART. XX.—*Influence of Arsenious Acid upon the Waste of the Animal Tissue.*

ACCORDING to experiments made by Prof. Schmidt and Dr. Stuerzwage of Dorpat,* arsenious acid when introduced into the circulation, occasions a considerable diminution of the ordinary waste of the tissues.

This decrease, which amounts to from twenty to forty per cent, occurs even after the administration of very small doses; more rapidly if the acid is injected directly into the veins; more slowly, yet with equal intensity, if absorbed from the intestines. The action is most striking in the case of fowls which neither vomit after injection of the arsenic nor reject their accustomed food; but even in cats which are subject to vomiting after the injection and must therefore be regarded as in a starving condition, the waste of the organism was diminished about twenty per cent after subtracting the decrease occasioned by the mere want of food.

This fact satisfactorily explains the fattening of horses after small doses of arsenious acid, a phenomenon well known to horse dealers.

An amount of fat and albuminous substances equivalent to the repressed carbonic acid and urea remains in the body and increases its weight, if the animal receives at the same time a sufficient amount of food.

When larger doses of arsenious acid are given nervous symptoms appear, which may be classified in two groups: spinal irritation and paralysis. To the first may be referred the vomiting, the accelerated respiration, the feeble pulse; to the last, the inclination to sleep, the weakness, and the retarded and labored breathing. Both may be explained by the very considerable congestion of the central organs which was constantly observed in post mortem examinations.

These experiments are of particular interest since they go far to prove the complete reliability of the published accounts of the custom of "arsenic eating," which is said to prevail among the peasantry of several Austrian provinces. These accounts have been time and again held up to ridicule by toxicologists,† and as a rule have been received with suspicion by all scientific men. They have nevertheless been widely published and are consequently well known to the public.

During the last eight or ten years the origin of these accounts

* *Journal für praktische Chemie*, 1859, lxxviii, p. 373.

† See for example, *Christison. Edinburgh Medical Journal*, Feb. 1856, i, 709. *A. Chevallier, Journal de Chimie Médicale, etc.*, 1854, [3.] x, 439. Or *Taylor*, in his work *On Poisons*. London, Churchill, 1859, p. 91.

has been generally attributed to Dr. v. Tschudi, who published a communication upon the subject in 1851;* an abstract of which may be found in Chambers' Edinburgh Journal, December 20, 1851, [N. S., No. 416,] p. 389.†

Two years later, v. Tschudi made another communication,‡ in support of his previous assertions:—this, in connection with his first letter which had previously attracted comparatively little attention, was very extensively copied.§

Similar stories had been circulated, however, long before the letters of v. Tschudi were made public. For example, our own attention was first directed to the subject by the statement published in the Penny Cyclopædia of the Society for the Diffusion of Useful Knowledge, London, 1832, ii, 403. Art. "*Arsenic, Medical Uses of.*" * * *

"That its [white oxyd of arsenic] employment in such doses [$\frac{1}{30}$ or $\frac{1}{15}$ of a grain] as we have stated is not only safe but beneficial, may be satisfactorily proved. Not only are old worn out horses endowed with new vigor, improved appetite, &c., by its use, but pigeons to which this article is given, show greater appetite and liveliness than others without it; and in Upper Styria the peasantry use it as a seasoning with many articles of food, such as cheese." * * *

In the Boston Medical and Surgical Journal, 1835, xii, 211, is the following:

"Dr. Strohmayer in his Medicinische Praktische,‖ relates in exemplification of the extent to which the system may become accustomed to the operation of arsenic, that a peasant who resided near a convent in the Tyrol, for a long time, took ten grains of arsenic daily with his food." * *

In noticing the article in Chambers' Journal, for 1851, a correspondent of the London and Edinburgh Monthly Journal of Medical Science, February, 1852, xiv, 190, cites the following extracts:

* *Wiener medizinische Wochenschrift*, October 11th, 1851, vol. i, No. 28.

† From which it was copied into Wells's Annual of Scientific Discovery, 1852, p. 362.—Hays's American Journal of the Medical Sciences for July, 1852, vol. xxiv, p. 270, also contains extracts of v. Tschudi's letter, taken from the French *Gazette des Tribunaux* through the *Journal des connaissances Med. Chirurg.*, December 16th, 1851.

‡ *Wiener medizinische Wochenschrift*, 1853, No. 1.

§ *In extenso* in *Journal de Chimie Médicale, etc.*, 1854, [3.] x, 439; from *La Presse Médicale Belge;* from *Journal de la Société des Sciences médicales et naturelles de Bruxelles;* abstract in Chambers's *Edinburgh Journal*, June 11th, 1853, [N. S.,] vol. xix, No. 493, p. 382.

An abstract of the first (1851) letter, in the *Gazette de Hôpitaux*, of Paris, May 16, 1854, p. 229; from *Journal de Médicine de Bruxelles*, (see also *London Medical Times and Gazette*, July 1854, xxx, 66,) is perhaps the best known of any of the numerous extracts from v. Tschudi's statements, unless it be that given by J. F. W. Johnston in his Chemistry of Common Life, New York, Appleton, 1855; i, 166; also in Blackwood's Magazine, Dec. 1853, lxxiv, 687.

[‖ Quere? Strohmayr, Frz. *Medicinische praktische Darstellung gesammelter Krankheitsfälle, u. des Heilverfahrens aus dem Tagebuch meiner Erfahrung Wien, Gerold*, 1831.]

"From Vogt's,* Arzneimittellehre, B. 1, S. 507." "It is well-known that old worn out horses gain an appetite, strength and spirit by the use of arsenic; and a pigeon which often got arsenic was observed to have its appetite increased and its movements more lively."

"From *Med. Jahb. des Oester. Staates*, 1822, S. 99." "There is scarce a district of Upper Styria in which in at least one house, arsenic may not be found under the name of Hydrach, Orpiment, &c. It is used for diseases of the domestic animals, against vermin, and also as a stomachic to increase the appetite. A peasant in my presence showed, with the point of a knife, how much arsenic he took daily and without which he said he could not live. I estimated the quantity at about two grains. It is also said to be used as a seasoning for cheese, and indeed several cases of poisoning by Styrian cheese have occurred, and one but lately."

Similar statements made by Wibmer, (probably in his book, entitled, "*Die Wirkung der Arzneimittel, u. Gifte im gesunden thierischen Körper*," 4 vols., Munich, 1831–39,) are referred to in German works upon the materia medica; while travellers who have spent much time in these provinces, all concur in their statements regarding the common custom of mixing arsenic with the food of horses.†

Evidence of this sort could without doubt be multiplied to almost any extent by any one familiar with the literature of the provinces in question, or with the habits of their people. A quantity of such material,‡ has indeed been recently collected by Heisch, and published in the London Chemical News, May 19, 1860, i, 280; from which we quote it, as being recent, (for the most part,) precise and tolerably direct; although it does not differ in its general import from the testimony which had already been offered.

On the Arsenic Eaters of Styria, by Charles Heisch, Esq., F.C.S., Lecturer on Chemistry at the Middlesex Hospital Medical College.§—At the last meeting of the Manchester Philosophical Society I observe that Dr. Roscoe called attention to the arsenic eaters of Styria. Having for the last two years been in communication with the medical men and other residents in the districts where this practice prevails, I shall feel obliged if you will allow me through your journal to make known the facts I have at present collected. The information is derived mainly from Dr. Lorenz, Imperial Professor of Natural History, formerly of Salzburg, from Dr. Carl Arbele, Professor of Anatomy in Salzburg, and Dr. Kottowitz, of Neuhaus, besides several non-medical friends. If human testimony be worth anything, the fact of the existence of arsenic eaters is placed beyond a doubt. Dr. Lorenz, to whom questions were first addressed, at once stated that he was aware of the practice, but added, that it is generally difficult to get hold of individual cases, as the obtaining of arsenic

* [Qu.? Voigtel, Fr. G. *System der Arzneimittellehre*, Leipzig, 1816.]

† A custom which seems also to prevail to a certain extent in England. Compare Kesteven, cited by Taylor, (*op. cit.*, p. 89.) from the Association Medical Journal, Sept. 6, and 20, 1856. We have to regret our inability to refer to K.'s original paper, the tenor of which is not readily to be inferred from Dr. Taylor's extracts.

‡ Compare Boner of Ratisbon in Chambers's Journal of Pop. Lit., &c., Feb. 9th, 1856, vol. v, No. 110, p. 90; see also *ibid*, July 19th, 1856, vol. vi, No. 133, p. 46.

§ From the Chemical News, May 19th, 1860.

without a doctor's certificate is contrary to law, and those who do so are very anxious to conceal the fact, particularly from medical men and priests. Dr. Lorenz was, however, well acquainted with one gentleman, an arsenic eater, with whom he kindly put me in communication, and to whom I shall refer again more particularly. He also says that he knows arsenic is commonly taken by the peasants in Styria, the Tyrol, and the Salzkammergut, principally by huntsmen and woodcutters, to improve their wind and prevent fatigue. He gives the following particulars:—

"The arsenic is taken pure when fasting, in some warm liquid, as coffee, beginning with a bit the size of a pin's head, and increasing to that of a pea. The complexion and general appearance are much improved, and the parties using it seldom look so old as they really are, but he has never heard of any case in which it was used to improve personal beauty, though he cannot say that it never is so used. The first dose is always followed by slight symptoms of poisoning, such as burning pain in the stomach and sickness, but not very severe.

"Once begun, it can only be left off by very gradually diminishing the daily dose, as a sudden cessation causes sickness, burning pains in the stomach, and other symptoms of poisoning, very speedily followed by death.

"As a rule, arsenic eaters are very long lived, and are peculiarly exempt from infectious diseases, fevers, &c., but unless they gradually give up the practice invariably die suddenly at last.

"In some arsenic works near Salzburg with which he is acquainted, he says the only men who can stand the work for any time are those who swallow daily doses of arsenic, the fumes, &c., soon killing the others. The director of these works, the gentleman before alluded to, sent me the following particulars of his own case. (This gentleman's name I suppress, as he writes that he does not wish the only thing known about him in England to be the fact that he is an arsenic eater; but if any judicial inquiry should arise which might render positive evidence of arsenic eating necessary, his name and testimony will be forthcoming):—

"'At seventeen years of age, while studying assaying, I had much to do with arsenic, and was advised by my teacher, M. Bönsch, Professor of Chemistry and Mineralogy at Eisleben, to begin the habit of arsenic eating. I quote the precise words he addressed to me:—'If you wish to continue the study of assaying, and become hereafter superintendent of a factory, more especially of an arsenic factory, in which position there are so few, and which is abandoned by so many, and to preserve yourself from the fumes, which injure the lungs of most, if not of all, and to continue to enjoy your customary health and spirits, and to attain a tolerably advanced age, I advise you, nay, it is absolutely necessary, that besides strictly abstaining from spiritous liquors, you should learn to take arsenic; but do not forget, when you have attained the age of fifty years, gradually to decrease your dose, till from the dose to which you have become accustomed you return to that with which you began, or even less.' I have made trial of my preceptor's prescriptions till now, the forty-fifth year of my age. The dose with which I began, and that which I take at present, I enclose; they are taken once a day, early, in any warm liquid, such as coffee, but not in any spirituous liquors.' The doses sent were No. 1, original dose, three grains: No. 2, present dose, twenty-three grains of pure white arsenic in coarse powder. Dr. Arbele says this gentleman's daily dose has been weighed there also, and found as above. Mr. —— continues:—'About an hour after taking my first dose (I took the same quantity daily for three months,) there followed slight perspiration with griping pains in the bowels, and after three or four hours a loose evacuation; this was followed by a keen appetite and a feeling of excitement. With the exception of the pain, the same symptoms follow every increase of the dose. I subjoin, as a caution, that it is not advisable to begin arsenic eating before the age of twelve or after thirty years.' In reply to my question, if any harm results from either interrupting, or altogether discontinuing the practice, he replies, 'Evil consequences only ensue from a long continued interruption. From circumstances I am often obliged to leave it off for two or three days, and I feel only slight languor and loss of appetite, and I resume taking the arsenic in somewhat smaller doses. On two occasions, at the earnest solicitations of my friends, I attempted entirely to leave off the arsenic. The second time was in January 1855. I was induced to try it a second time from a belief that my first illness might have arisen from some other cause. On the third day of the second week after leaving off the dose I was attacked with faintness, depression of spir-

its, mental weakness, and a total loss of the little appetite I still had; sleep also entirely deserted me. On the fourth day I had violent palpitation of the heart, accompanied by profuse perspiration. Inflammation of the lungs followed, and I was laid up for nine weeks, the same as on the first occasion of leaving off the arsenic. Had I not been bled, I should most likely have died of apoplexy. As a restorative, I resumed the arsenic eating in smaller doses, and with a firm determination never again to be seduced into leaving it off, except as originally directed by my preceptor. The results on both occasions were precisely the same, and death would certainly have ensued had I not resumed arsenic eating.' One of the most remarkable points in this narrative is that this gentleman *began* with a dose which we should consider poisonous. This is the only case of which I have been able to obtain such full particulars, but several others have been mentioned to me by those who knew the parties, and can vouch for their truth, which I will briefly relate.

"One gentleman, besides stating that he is well aware of the existence of the practice, says he is well acquainted with a brewer in Klagenfürth who has taken daily doses of arsenic for many years; he is now past middle life, but astonishes every one by his fresh juvenile appearance; he is always exhorting other people to follow his example, and says, 'See how strong and fresh I am, and what an advantage I have over you all! In times of epidemic fever or cholera, what a fright you are in, while I feel sure of never taking infection.'

"Dr. Arbele writes, 'Mr. Curator Kürsinger (I presume curator of some museum at Salzburg,) notwithstanding his long professional work in Lungau and Binzgau, knew only two arsenic eaters, one the gentleman whose case has just been related, the other the ranger of the hunting district in Grossarl, named Trauner. This man was at the advanced age of 81 still a keen chamois hunter and an active climber of mountains; he met his death by a fall from a mountain height while engaged in his occupation. Mr. Kürsinger says he always seemed very healthy, and every evening regularly, after remaining a little too long over his glass, he took a dose of arsenic, which enabled him to get up the next morning perfectly sober and quite bright. Professor Fenzl of Vienna was acquainted with this man, and made a statement before some learned society concerning him, a notice of which Mr. Kürsinger saw in the *Wiener Zeitung*, but I have not been able to find the statement itself. Mr. Krum, the pharmaceutist here, tells me that there is in Stürzburg a well known arsenic eater, Mr. Schmid, who now takes daily twelve, and sometimes fifteen, grains of arsenic. He began taking arsenic from curiosity, and appears very healthy, but always becomes sickly and falls away if he attempts to leave it off. The director of the arsenic factory before alluded to is also said to be very healthy, and not to look so old as forty-five, which he really is.'*

"As a proof how much secrecy is observed by those who practise arsenic eating, I may mention that Dr. Arbele says he inquired of four medical men, well acquainted with the people of the districts in question, both in the towns and country, and they could not tell him of any individual case, but knew of the custom only by report.

"Two criminal cases have been mentioned to me, in which the known habit of arsenic eating was successfully pleaded in favor of the accused. The first, by Dr. Kottowitz of Neuhaus, was that of a girl taken up in that neighborhood on strong suspicion of having poisoned one or more people with arsenic, and though circumstances were strongly against her, yet the systematic arsenic eating in the district was pleaded so successfully in her favor, that she was acquitted, and still lives near Neuhaus, but is believed by every one to be guilty. The other case was mentioned by Dr. Lorenz. A woman was accused of poisoning her husband, but brought such clear proof that he was an arsenic eater, as fully to account for arsenic being found in the body. She was, of course, acquitted.

"One fact mentioned to me by some friends is well worthy of note. They say: 'In this part of the world, when a graveyard is full, it is shut up for about twelve years, when all the graves which are not private property by purchase are dug up, the bones collected in the charnel-house, the ground ploughed over, and burying begins again. On these occasions the bodies of arsenic eaters are found almost un-

* The man above mentioned seems quite to differ with Mr. —— on the impropriety of taking arsenic with spiritous liquors, and actually employs it as a means of correcting their effects. All others that I have heard of concur in saying that it should be taken fasting.

changed, and recognisable by their friends. Many people suppose that the finding of their bodies is the origin of the story of the vampire.' In the *Medicinischer Jahrbuch des Oster. Kaiserstaates*, 1822, *neuest Folge*, there is a report by Professor Schallgruber, of the Imperial Lyceum at Grätz, of an investigation undertaken by order of government into various cases of poisoning by arsenic. After giving details of six post-mortem examinations, he says:—'The reason of the frequency of these sad cases appears to me to be the familiarity with arsenic which exists in our country, particularly the higher parts. There is hardly a district in Upper Styria where you will not find arsenic in at least one house under the name of hydrach. They use it for the complaints of domestic animals, to kill vermin, and as a stomachic to excite an appetite. I saw one peasant show another, on the point of a knife, how much arsenic he took daily, without which, he said, he could not live; the quantity I should estimate at two grains. It is said, but this I will not answer for, that in that part of the country this poison is used in making cheese; and, in fact, several cases of poisoning by cheese have occurred in Upper Styria, one not long since. The above-mentioned peasant states, I believe truly, that they buy the arsenic from the Tyrolese, who bring into the country, spirits and other medicines, and so are the cause of much mischief.' This report is, I believe, mentioned in Orfila's *Toxicology*, and one or two other works, but I have not seen it quoted myself; it is interesting, as being early and official evidence of arsenic eating. Since I received the above information, a gentleman who was studying at this hospital, told me that, when an assistant in Lincolnshire, he knew a man who began taking arsenic for some skin disease, and gradually increased the dose to five grains daily. He said he himself supplied him with this dose daily for a long time. He wrote to the medical man with whom he was assistant, and I have been for a long time promised full particulars of the case, but beyond the fact that he took five grains of arsenic, in the form of Fowler's solution, daily, for about six years, and could never leave it off without inconvenience and a return of his old complaint, I have as yet not received them. I have delayed publishing these facts for some time, hoping to get information on some other points, for which I have written to my friends abroad; but as considerable delay takes place in all communications with them, I have thought it better to publish at once the information I have already received. All the parties spoken of are people on whom the fullest reliance can be placed, and who have taken much pains to ascertain the foregoing particulars. The questions which still remain unanswered are these:—

"1st. Can any official report be obtained of the trials of the two people mentioned by Drs. Kottowitz and Lorenz?

"2nd. Do medical men in these districts, when using arsenic medicinally, find the same cumulative effects as we experience here? Or is there anything in the air or mode of living which prevents it?

"3rd. Can any evidence be obtained as to how much of the arsenic taken is excreted? to show whether the body gradually becomes capable of enduring its presence, or whether it acquires the power of throwing it off?*

"I have proposed to the gentleman who furnished me with the particulars of his own case either to make an estimate of the arsenic contained in his own urine and fæces during twenty-four hours, or to collect the same and forward them to me that I may do so, but as yet have received no answer."—*Pharmaceutical Journal.*

The only wonder is that direct experiments have not been made long ago upon the excretions of suspected arsenic eaters, or upon their bodies after death. In calling attention to the subject some months since, (*Répertoire de Chimie Appliquée*, February, 1860, ii, 44,) we took occasion to urge this point and to bring forward the observation of Prof. E. Kopp†, who found in the course of his experiments upon arsenic acid—which was manufactured upon the great scale and largely employed in calico-print-

* The fact of the preservation of the bodies shows that some considerable quantity must be retained [or rather offers an example of the well known fact that corpses loaded with fat decompose but slowly.—F. H. S.]

† Comptes Rendus, 1856, xlii, 1063; *J. pr. Ch.* lxix, 273.

ing by him—that the weight of his body rapidly increased, some 20 lbs. having been gained in the course of the two months, during which he was subject to absorb the acid, his hands having been frequently in contact with the arsenical solution: arsenic being detected the while in his solid and liquid excrements. As soon, however, as the exposure to the arsenic ceased, his weight began to decrease, and in the course of 9 or 10 weeks, fell back again to its normal—150 lbs. Believing that direct, positive evidence like this—though the instance be solitary—where the subject of the experiment was a healthy, vigorous man, and a trained observer, ought to outweigh almost any amount of negative testimony, such as has been brought forward by physicians who have not witnessed similar effects upon their *diseased* patients when the latter were treated with arsenical preparations. It should be observed that one of the strongest arguments brought up by toxicologists against the truth of the accounts of the arsenic eaters has been drawn from the result of medical practice. Without attempting to discuss the matter at length, it may, nevertheless be permitted to the non-professional reader of the medical literature bearing upon this subject to remark that the evidence there accumulated in spite of its apparent obscurity and of the contradictions with which it is involved seems to point clearly to the fact that in minute doses arsenious acid acts beneficially upon the general health of many patients. Thus, according to Dr. Henry Hunt:*

"Arsenic operates most favorably on persons who are of lax fibre, accompanied by a languid state of the circulation and whose secretions are rather profuse than otherwise; the urine pale and plentiful, and more especially on those whose skin is cold and moist. In persons of this description, whilst arsenic to an extent far beyond other medicines, relieves the neuralgic pain, it improves the general health, and gives firmness and vigor to the constitution." * * * * While upon certain peculiar temperaments it is hurtful; thus Dr. Hunt continues, "when neuralgia is associated with the same morbid action in the spine, with anæmia; or arises from injuries of nerves; or local irritation of nerves by diseases, or unnatural growth of bone; or if it be complicated with engorgement of the liver and other viscera; arsenic is usually injurious, and I believe seldom useful."

Again, not to multiply instances further, Erichsen,† in treating of the use of arsenic in diseases of the skin, "lays great stress upon the necessity of attending particularly to the constitution, and temperament of the patient before commencing the use of the medicine. It will be badly borne by individuals of a plethoric habit of body or of a highly sanguine or sanguineo-nervous temperament,—this arises from the stimulating properties of the metal. In such cases, the digestive organs become so ir-

* In his work upon Neuralgic Disorders, cited in Braithwaite's Retrospect of Pract. Med., 1844, No. ix, p. 34; also, *ibid.* No. x, p. 23.

† Braithwaite's Retrospect, 1843, No. viii, p. 14.

ritated and the nervous system so excited under the use of the arsenic that it is impossible to employ it in any such dose as can be expected to produce a beneficial effect upon the cutaneous affection. There are other circumstances which contra-indicate the use of this remedy, namely, 'the complication of the cutaneous affection with other diseases,' and especially with irritative or inflammatory gastric dyspepsia, accompanied with a sensation of heat and oppression at the epigastrium, increased by food so well described by Dr. Todd. When this form of dyspepsia is present the smallest doses of arsenic will do harm, as the usual effect of the remedy when continued too long is to produce these very symptoms. Besides this form of indigestion any other local inflammatory condition of the system or the superinvention of phthisis, will contra-indicate the use of so powerfully stimulating a tonic as arsenic." Further on he remarks: "It has already been shown that the use of the preparations of this metal is exceedingly hazardous in individuals of a sanguine or sanguineo-nervous temperament and excitable habit of body, or in those who suffer from or are peculiarly disposed to irritative gastric dyspepsia, or any inflammatory disorder. On the other hand, they are in most cases borne well by individuals of a somewhat phlegmatic, debilitated, or lax habit of body, more particularly, if they are past the middle age, with a pale cachectic complexion, languid weak circulation, and a general want of tone about the system, acting upon such patients as powerful and useful tonics. In persons of this habit of body the diseases of the skin appear rather to be dependent upon a degree of debility or want of power in the cutaneous capillaries; and it is in these patients that the preparations of arsenic are of great service in exciting in a peculiar manner a more healthy action in this class of vessels, thereby modifying or removing those morbid changes that are the results of an abnormal condition in their secernent and nutrient functions."

In this connection it must not be forgotten that in the opinion of many scientific men, the healing action of various mineral waters may depend, in part, at least, upon the arsenic which these springs are known to contain.* A doctrine which is publicly taught by several of the chemical Professors at Paris.

Taken as a whole the medical evidence which has fallen under our notice, is adverse to the possibility of "arsenic eating," only in so far as relates to the large quantities of the poison which, as is affirmed, the human body can accustom itself by long continued habit, to support with impunity. This last enquiry, however interesting in itself, is one on which very little is known with certainty as yet, and is plainly of quite secondary importance in a scientific point of view to that of the beneficial action of moderate doses of arsenious acid, which would now appear to be proved. From the very general interest which attaches to the subject it is greatly to be hoped that further researches may soon decide the amount of this tolerance. F. H. S.

* Compare *Walchner, Ann. Ch. u. Pharm.*, lxi, 206; or *Comptes Rendus*, xxiii, 614. *Figuier, Comptes Rendus*, xxiii, 820. *Chevallier* and *Schanefele, ibid.* xxv, 750. *Thenard, ibid.* xxxix, 769.

Art. XXI.—*Geographical Notices.* No. XIII.

Journal of the American Geographical Society.—The first number of the second volume of the American Geographical Society's Journal has appeared in a new and enlarged form, an octavo volume of 148 pages. The articles (eight in number) are of a more extended and scientific character than usual, and the whole appearance of the Journal is such as will reflect great credit on the society and enlarge its sphere of usefulness.

Article first, which is compiled from data furnished by the Hydrographical Office, Washington, gives an account of the progress of Marine Geography, within the past few years; which is followed by a synopsis of the operations of the Coast Survey during the year 1859, by Prof. Bache. An account of the lake Yojoa in Honduras, contributed by Mr. E. G. Squier, is the next article. The address commemorative of Karl Ritter, delivered before the Society by Prof. Guyot, comes next in order, forming the most complete and eloquent tribute to the great geographer which has yet appeared. Its review of the characteristics and contents of Ritter's Erdkunde is especially valuable. The sixth article is a translation by Mr. E. R. Straznicky from the Journal of the Geographical Society of France, of an essay on the Geographical distribution of Animals, by Mons. A. Maury. Mr. J. G. C. Kennedy, the superintendent of the U. S. Census, then reviews the origin and progress of Statistics, and Dr. Wynne illustrates the working of benevolent societies, such as the Odd Fellows, among the laboring classes. The number is concluded by an excellent and full survey of recent geographical and statistical literature, prepared by the General Secretary of the Society, Mr. D. W. Fiske, to whom, with the coöperation of the Committee of Publication, the editing of the Journal was entrusted. The Society now has a small but well selected library, with rooms in a central part of New York City. Its list of active members enrolls about five hundred names, and its usefulness and importance have never been greater than at present.

Schlagintweit's Mission to Central and High Asia.—We have received through Mr. S. H. Grant of New York, the prospectus of Mr. Brockhaus of Leipsic, announcing the contents and character of the Report which is soon to be published by the brothers Schlagintweit on their journey to the Himalayas, from 1854 to 1858. It contains some information to which we have not had access in our previous notices of their expedition. Since the return of the authors from India in June 1857, they have been engaged in preparing for publication the results

of their observations, and are now able to promise a work in nine volumes, quarto, with an atlas in three folio volumes. The importance of this work will justify us in explaining its character at some length.

The first volume will be devoted to Astronomy and Magnetism. The observations extend from Ceylon to Turkistan and from Assam to Kabul. Their importance may be the better appreciated if it is borne in mind that "with the exception of the well known observations at the government observatories of Bombay and Madras, some very valuable ones by Taylor and Caldecott in Southern India, and others recently made by Brown at Travankor, scarcely any observations have been taken in the interior of India, so that the modifications of the magnetic lines over this large area form a new object of scientific discussion."

In India proper the astronomical labors of the Schlagintweits relate chiefly to the determination of the true and magnetic meridian and to observations for finding time. But in the Himalaya, their operations included also determinations of latitude and longitude. The whole of Western Thibet has been found to be farther west than has hitherto been supposed, and for Kuenluen and Turkistan the latitudes have also been largely corrected.

The second volume will contain the hypsometrical and trigonometrical observations, including the determination of about 2000 points in the various countries explored. The third volume, on Topical Geography, has for its object a practical aim, reviewing chiefly the commercial and military routes in High Asia, with reference to their commercial and military importance. Part of this volume is devoted to linguistic researches and vocabularies. The fourth and fifth volumes include all that the explorers have collected on Meteorology, and the sixth is devoted to Geology. Volume seventh relates to Botany and Zoology. Volume eighth is given to Ethnography, including an examination and comparison of the facial casts to which we have previously referred in this Journal. The ninth and last volume presents in a popular form comparative descriptions of the various regions of India and High Asia.

The Atlas will contain: 1. Maps, geographical, physical, and geological; 2. Profiles, meteorological, hydrographical and geological; and 3. Views and general panoramas. The general size of the plates is three feet by two. The whole cost of the work will be £36, and its completion is promised in about three years.

Canadian Expedition to the Red River under Gladman, Dawson, Hind, and Napier, 1857–1858.—The interest which has been manifested in the report of the Palisser expedi-

tion contained in a recent number of this Journal,* leads us to condense and translate from Dr. Petermann's excellent *Mittheilungen* (January, 1860) an account of the explorations of the Red River which were made in 1857 and 1858 by Gladman, Dawson, Hind, and Napier. We regret that we cannot reproduce the admirable maps which accompany the article. The writer in Petermann's Journal remarks substantially as follows:

Although the Canadians had long endeavored to direct the attention of the British government to that vast portion of British North America, which stood until very recently under the immediate supervision of the Hudson's Bay Company, and had tried to induce them to effect a revision of the claims of that mercantile body, it was nevertheless, not until 1856 when gold was discovered in Fraser's and Thompson's rivers, that the British government took the matter into serious consideration, and in 1857 sent out an expedition (Pallisser's expedition) and declared in 1858 New Caledonia, as it was called under the above mentioned company, an independent colony, to be known in future by the name of British Columbia. At the same time it was urged, that the government of Canada might be empowered to incorporate adjacent portions of land, particularly the so-called Saskatchewan district, east of the Rocky Mountains. This expedition accomplished its chief object, to find a passage across the Rocky Mountains, and also reported favorably in regard to future settlements in the Saskatchewan district, which may be called the intermediate district between the settled portion of British North America and the new gold region in British Columbia. At the same time with Palliser's expedition another expedition was started directly by the Canadian government, and it is our object in the present paper, after having presented a few general remarks on the country, to give a brief synopsis of the course of this latter expedition.

The Saskatchewan district between the Red River and the Rocky Mountains has already, since the beginning of the present century, been the object of many explorations, the most prominent of which are those of Astronomer Thompson†, Lefroy, Richardson, Lord Selkirk, Blodget, and others. They all agree that the Saskatchewan district is well adapted for cultivation. It comprises an immense area, and as early as 1805, Lord Selkirk

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* Vol. xxviii, p. 320.

† Thompson was from 1790 over 30 years in the employ of the Hudson's Bay Company, and the reports of his explorations (37 vols.) are deposited in the Archives of this Company. From fragments of them it appears that Thompson possessed a great knowledge of the country, but it is doubtful whether these reports will ever be accessible to such as are not connected with the Company. Until now the Company has kept them back. [Compare this Journal, xxviii, 344, note.]

said that it could give bread to at least 30 millions of people. In regard to the climate, says Blodget, who is most thoroughly acquainted with the subject, that the average temperature in winter is not below that of St. Petersburg and Moscow; in summer it equals that of northern Italy and New York. The temperature increases, just as in Europe, as you go from east to west. Spring commences at all points almost at the same time. There is no want of rain; grass, forests and buffaloes abound. Useful timber is abundant; coal is found in many places, but particularly rich deposits exist at the foot of the Rocky Mountains, and near the Little Sauris River. The country is level and appears so even, that Blakistone remarked that for the construction of a railroad nothing was required but to put down the rails. Its numerous lakes and rivers can easily be connected for internal communication, and afford even now the only means of transport between the different stations of the Hudson's Bay Company. The Saskatchewan district can also easily be connected with the new gold region by means of commodious roads through Palliser's passage across the Rocky Mountains. This new colony will, by reason of its very favorable situation, its beautiful harbors, but particularly by reason of its wealth in gold, surely rise as speedily as Southern California; and, as it is less capable of agriculture, would naturally become the great market for the products of its eastern neighbors, in the Saskatchewan district.

We may therefore well be justified in prognosticating for this district a prosperous future in regard to agriculture, but we cannot agree with such opinions expressed some time ago in the Montreal Pilot, that by a regularly established road from Lake Superior to Lake of the Woods, Red River, Lake Winnipeg, Saskatchewan river, across the Rocky Mountains to the rivers of British Columbia, thence to the Pacific, all commercial intercourse between Europe and China, Japan and India would take this route. A road which changes so often between land and water can never become a general commercial road for such a distance, not to mention the almost insurmountable difficulties for vessels of a larger draught, such as sudden bends, rapids, falls, shallow waters, etc., and the entirely uncultivated state of the country.

After these few remarks we return to our subject proper. We can give but a brief synopsis, and refer those who desire a detailed account of the Canadian expedition, to the "Reports on the exploration of the country between Lake Superior and the Red River Settlement." A still more minute account is given in the "Papers relative to the Explorations of the Country between Lake Superior and the Red River settlement, presented to both Houses of Parliament, London, 1859." Three charts by

Hind (one a reprint of Thompson's), and a sketch of the regions, which Dawson traveled through, by himself, appeared at the same time.

The members of the Canadian expedition landed July 31, 1857, at Fort William, and started in boats along the usual route of the Hudson's Bay Company for Lake Winnipeg, in order to ascertain the practicability of this route. To this end surveys of rivers were made and a very minute determination of levels. Napier estimates the whole length of the route to be 747 miles, viz: from Lake Superior to Rainy Lake 335 miles; thence to Rat Portage at the northern extremity of Lake of the Woods 176 miles; from this point to Fort Garry on the Red River 236 miles. Of these three portions only the middle one, upon Rainy Lake, which is at an average 460 feet wide and 6 feet deep, forms a continuous water road. Its falls (Chaudiere falls near Fort Francis, 22 feet,) may, according to Dawson, easily be made harmless by two water gates. The two remaining portions of the route can only be travelled by land, unless one prefers the tedious transport from one little river to another. The Kaministiquoria on the first portion of the route cannot be navigated, as its rapids, shallow water places, and falls (Kakabeke falls, 119 feet,) are too numerous. From Little to Great Dog Lake, a distance not over a mile, this river falls 348 feet, and yet the portage in this place has still an elevation of 142 feet over Great Dog Lake. This is the steepest descent on the whole route. The passage upon Dog River is partially obstructed by rocks and sandbanks, and on Prairie Portage, between Lake Superior and Lake Winnipeg, it leads mostly through swamps. The difference of elevation between Lake Superior and Prairie Portage, 54 miles distant from one another, is, after Dawson, 879 feet, according to Napier 887 feet; that between Prairie Portage and Lake Winnipeg (325 miles) is calculated by Dawson 892, by Napier 870 feet. Thus the descent toward the east is much more rapid than toward the west. The canoe route from Savannah River to Rainy Lake has too many portages and the Rivière la Seine is, by reason of the numerous difficulties in its course, entirely objectionable. But the Winnipeg River, from Lake of the Woods to Lake Winnipeg, was by all declared to be the most difficult and impracticable on the whole route. The canoe route on the Pigeon River, from Lake Superior to Rainy Lake along the boundary, is the shortest, but it has 29 portages, of which many lead through United States territory. Another route to the Red River, which is still used by the Hudson Bay Company, commences from Fort York, near Hudson's Bay, and goes up Hays River, through Knee and Holy Lakes, Wepinapanis River, White Water Lake and Sea River, down to Lake Winnipeg; but it requires three weeks of hard work to travel it, besides the ac-

cess to Fort York, through Hudson's Bay is only about two months open during the year. But the most commodious and most frequented road to the Red River over St. Paul and Crow Wing leads entirely through United States territory. In the English possessions the best connection between Lake Superior and the Red River would be established by country roads, the one from Lake Superior to Rainy Lake, the other from Lake of the Woods to the Red River. In regard to the first, however, nothing has as yet been done, and only in the latter district explorations have been made with this view. When Gladman had arrived at Fort Garry (September, 1857,) he sent out engineers Napier and Dawson to reconnoitre this hitherto entirely unknown district, which explorations were continued by Gaudet and Wells during the winter 1857–1858.

The whole country between the Red River and Lake of the Woods appeared perfectly level, although it actually descends toward the east nearly 400 feet. Dry prairies change alternately with wooded districts and extensive swamps, the latter being particularly frequent toward north. The establishment of a road through this district seemed to them an utter impossibility.

Hind went up the Assiniboine River, explored the Great and Little Rat river, examined the valley of the Red river up to Pembina, and followed the Reed Grass or Roseau river up to a great swamp, which separated this stream from a lake of the same name. Unfortunately Hind could not survey this river up to its sources, but all the Indians who lived there agreed that a swamp of 9 miles in extent existed between Roseau lake and Lake of the Woods. This swamp sends the Reed river, 30 miles long, to the latter lake, and another little rapid river, about 40 or 50 miles long, to lake Roseau. From the Great Muskeg morass goes a little river westward into an extensive swamp, from which the Rat river issues.

Gladman was relieved from his post as chief in April, 1858, and Napier also recalled about this time. But Hind went the same spring again with Dickinson, Fleming and Hine on another expedition known as the "Assiniboine and Saskatchewan expedition." Their object was to explore the regions west of the Red River and Lake Winnipeg up to the Saskatchewan river. Before they arrived at Fort Garry, Dawson, Wells and Gaudet had already made some new surveys, around the Red river, Lake Winnipeg, and the lower Assiniboine, and had just left for the lake district. This latter party went by way of Lake Manitobah and Lake Winnipego-sis, over Mossy Portage toward Cedar or Bourbon lake to the grand rapids of the Saskatchewan river. At Mossy Portage they separated; Wells went over Lake Winnipego-sis, Lake Dauphin, Lake Manitobah, the Little Saskatchewan river, which he found to be 8 to 12 feet deep, 250 yards wide,

free from rapids and throughout adapted for steam navigation, thence over Lake Winnipeg to the Red river. The rest of the party followed Swan river to Fort Pelly, and thence went down the Assiniboine river.

Dawson considers the whole alluvial plain east of the Pasquia and Porcupine hills and Dauphin mountains, where the large lakes are situated, well adapted for settlements. It is partly prairie land, for the most part, however, thickly wooded. North of Lake Dauphin wood predominates; south of it the country becomes more open, and toward the Assiniboine an apparently endless prairie commences. Wheat gives abundant harvests near Lake Manitobah and the Little Saskatchewan river, and near the latter even Indian corn may be cultivated. The valley of the Swan river is particularly fertile and its climate equals that of the Red river district. The Red Deer river district has also a good soil and fine climate, as its maple tree forests plainly show. Coal is said to be found in the Porcupine hills and the Duck mountain; Dawson himself found samples of lignite near Snow river. The great alluvial valley of the Assiniboine and its branches will, in his opinion, hereafter become one of the finest wheat growing districts upon earth. Near Moss or Dauphin river, a fine navigable stream, the Indians grow maize, melons and potatoes. Vines, hops, and vetches grow naturally in abundance.

Hind and his companions went (June 14, 1858,) from Fort Garry in a westerly direction over Fort Ellis toward the missionary station near Qu' Appelle lake (July 18), where he divided his corps into three parties: Dickinson traveled on the Qu' Appelle river up to its mouth, thence on horseback to Fort Pelly; Hine surveyed Long lake northwest of the Qu' Appelle mission, then went over land to Fort Pelly to meet Dickinson, and to explore with him the Dauphin mountains; Hind and Fleming followed the Qu' Appelle river up to its source, went over to the elbow of the southern arm of the Saskatchewan or Bow river, on which they travelled down until they reached Fort à la Corne (Aug. 9). The Qu' Appelle and Bow rivers have no connection as Dr. Hector believes. The latter (southern arm of the Saskatchewan) has down from its elbow for a distance of about 100 miles, a width of 300 yards to half a mile, then it becomes narrower and straighter in its course, its sand and mud banks disappear and finally it hurries through a narrow and deep valley, with a strong current toward the northern arm of the Saskatchewan, with which it unites forming one river (Saskatchewan), which now goes toward Fort à la Corne through Pine and Cedar lakes into Lake Winnipeg. Fleming followed this course from Fort à la Corne into Lake Winnipeg, along its western coast, until he reached the Red River. Hind made a

land voyage along Long creek, then turning southeast went over Touch Wood hills to Fort Ellis, where he met Dickinson with whom he returned over White Mud river to Fort Garry (September 4).

But Hind and Fleming soon started on another excursion (September 18). They went in boats along the western shores of Lake Winnipeg, up to the mouth of the Little Saskatchewan, hence (September 29) into Lake Manitobah, and by means of Water Hen river and a lake of the same name reached Lake Winnipego-sis, where they examined the salt springs, which had been imprudently exhausted by the Indians. From here they started for Lake Dauphin, ascended the Dauphin mountains (1700 feet high), and navigated Lake Manitobah in different directions. Hind stayed four days on a little island there, which was much revered by the Indians as the seat of the "Manitou," or fairies. On its northern side were limestone cliffs about fifteen feet high, which by the beating of the waves emitted sounds very similar to chimes from a number of church bells, ringing at a distance. From Oak Point, at the southern extremity of the lake, the party went over land toward Fort Garry, where they arrived the 31st of October, 1858.

Hine, while sojourning on the Red river during the fall months, took photographic views of landscapes, churches, Indians, etc. Dickinson made excursions in the district east of the lower Red River, and in the regions between the Assiniboine and the U. S. boundary, but particularly along Rivière Sal through the Pembina mountains and Blue hills.

Some Canadian journals have blamed this Expedition for not having made any determination of points and for giving generally but little positive information, although $50,000 to $60,000 had been expended for the purpose. They said that the whole country had been much better explored by the late astronomer Thompson. This, however, is an unjust imputation. Astronomical observations of points, although very valuable, cannot be the main object of explorers, who have to run through a great number of districts in a comparatively very short time, and who must give us the general features of the country; moreover, as here a great number of such fixed points already exist, a careful survey of routes by dead reckoning is perfectly sufficient. The reproach that the country had been much better explored by Thompson is most unjust. Thompson's reports were undoubtedly as little accessible to the members of the Canadian expedition as they were to the rest of the world; besides, if we compare Thompson's chart with that of the expedition of 1858, we perceive that our knowledge of the country between Lake Winnipeg and Bow river is more accurate and more complete than Thompson's.

The expedition has achieved much. They made very comprehensive levellings, effected numerous measurements of width, depth and rapidity of rivers and lakes, made geological observations, inquired into the climate, forests, quality of soil, etc., made surveys and discoveries between Lake of the Woods and the Red river, between the Assiniboine river and the U. S. boundary, along the upper Assiniboine and Qu' Appelle rivers, in the district of the great lakes, etc. A comparison of their charts with the older ones of these districts will at once show that the money was not thrown away.

This expedition has moreover excited the curiosity of the people more than that under Capt. Palliser. Thus a society was formed at St. Paul in Minnesota, who, under the direction of Col. Nobles, left this city in June, 1859, with the object to explore the valleys and sources of the Saskatchewan and Columbia rivers. Their plan was, to start from the elbow of Bow river toward the Rocky Mountains, to explore carefully the region of their eastern foot up to Edmonton House, thence to go over Athalaska Portage between Mount Hooker and Mount Brown toward the sources of Thompson's river and here to disperse in different directions. Col. Nobles intended to start for the sources of Columbia river, and to return over Lewis and Clarke's Passage, the Missouri Falls, the valley of the Milk river, Fort Mandan, Big Stone Lake, and Fort Ridgley to St. Paul. Dr. Goodrich accompanies them as physician, and the Smithsonian Institution sent Dr. C. L. Anderson, of Minneapolis, to make scientific observations and collections.

The "Board of Trade" in St. Paul offered a reward of $1000 for the first steamer that should ply on or before the first of June on the Red river, and the "Anson Northup" really commenced her voyages in June. She carries, besides passengers, 100 to 150 tons of cargo, and is intended to do the post service between the mouth of the Shagerme river and Fort Garry, and thus to connect St. Paul, (which sustains a post wagon up to the Shagerme river,) directly with the Red river.

Another company in Canada intend to put four steamers on Rainy lake, Red river and Lake Winnipeg. Even the settlers on the Red river themselves show an active spirit of progress.

ART. XXII.—*Discussion between two Readers of Darwin's Treatise on the Origin of Species, upon its Natural Theology.*

FIRST READER.—Is Darwin's theory atheistic or pantheistic? or, does it tend to atheism or pantheism? Before attempting any solution of this question, permit me to say a few words tending to obtain a definite conception of *necessity*, and *design*, as the sources from which events may originate, each independent of the other; and we shall, perhaps, best attain a clear understanding of each, by the illustration of an example in which simple human designers act upon the physical powers of common matter.

Suppose, then, a square billiard table to be placed with its corners directed to the four cardinal points. Suppose a player standing at the north corner, to strike a red ball directly to the south; his design being to lodge the ball in the south pocket; which design, if not interfered with, must, of course be accomplished. Then suppose another player, standing at the east corner, to direct a white ball to the west corner. This design also, if not interfered with, must be accomplished. Next suppose both players to strike their balls at the same instant, with like forces, in the directions before given. In this case the balls would not pass as before, namely, the red ball to the south, and the white ball to the west, but they must both meet and strike each other in the centre of the table, and, being perfectly elastic, the red ball must pass to the west pocket, and the white ball to the south pocket. We may suppose that the players acted wholly without concert with each other, indeed they may be ignorant of each other's design, or even of each other's existence; still we know that the events must happen as herein described. Now the first half of the course of these two balls is from an impulse, or proceeds from a power, acting from design. Each player has the design of driving his ball across the table in a diagonal line to accomplish its lodgment at the opposite corner of the table. Neither designed that his ball should be deflected from that course and pass to another corner of the table. The *direction* of this second part of the motion, must be referred entirely to *necessity*, which directly interferes with the purpose of him who designed the rectilinear direction. We are not in this case, to go back to find design in the creation of the powers or laws of inertia, and elasticity, after the order of which the deflection, at the instant of collision, necessarily takes place. We know that these powers were inherent in the balls, and were not created to answer this special deflexion. We are required, by the hypothesis, to confine attention in point of time, from the instant preceding the impact of the balls, to the time of their

arrival at the opposite corners of the table. The cues are moved by design. The impacts are acts from design. The first half of the motion of each ball is under the direction of design. We mean by this the particular design of each player. But at the instant of the collision of the balls upon each other, direction from design ceases, and the balls no longer obey the particular designs of the players, the ends or purposes intended by them are not accomplished, but frustrated, by *necessity*, or by the necessary action of the powers of inertia and elasticity, which are inherent in matter, and are not made by any design of a Creator for this special action, or to serve this special purpose, but would have existed in the materials of which the balls were made, although the players had never been born.

I have thus stated, by a simple example in physical action, what is meant by design and what by necessity; and that the latter may exist without any dependence upon the former. If I have given the statement with what may be thought, by some, unnecessary prolixity, I have only to say that I have found many minds to have a great difficulty in conceiving of necessity as acting altogether independent of design.

Let me now trace these principles as sources of action in Darwin's work or theory. Let us see how much there is of design acting to produce a foreseen end, and thus proving a reasoning and self-conscious Creator; and how much of mere blind power acting without rational design, or without a specific purpose or conscious foresight. Mr. Darwin has specified in a most clear and unmistakeable manner the operation of his three great powers, or rather, the three great laws by which the organic power of life, acts in the formation of an eye. (See p. 169). Following the method he has pointed out, we will take a number of animals of the same species, in which the eye is not developed. They may have all the other senses, with the organs of nutrition, circulation, respiration and locomotion. They all have a brain and nerves, and some of these nerves may be sensitive to light; but have no combination of retina, membranes, humors, &c., by which the distinct image of an object may be formed and conveyed by the optic nerve to the cognizance of the internal perception, or the mind. The animal in this case would be merely sensible of the difference between light and darkness. He would have no power of discriminating form, size, shape, or color, the difference of objects, and to gain from these a knowledge of their being useful or hurtful, friends or enemies. Up to this point there is no appearance of *necessity* upon the scene. The billiard balls have not yet struck together, and we will suppose that none of the arguments that may be used to prove, from this organism, thus existing, that it could not have come into form and being without a creator acting to this end with intelligence

and design, are opposed by anything that can be found in Darwin's theory; for so far, Darwin's laws are supposed not to have not come into operation. Give the animals thus organized, food and room, and they may go on, from generation to generation, upon the same organic level. Those individuals that, from natural variation, are born with *light-nerves* a little more sensitive to light than their parents, will cross or interbreed with those who have the same organs a little less sensitive, and thus the mean standard will be kept up without any advancement. If our billiard table were sufficiently extensive, *i. e.*, infinite, the balls rolled from the corners would never meet and the *necessity* which we have supposed to deflect them would never act.

The moment, however, that the want of space or food commences *natural selection* begins. Here the balls meet, and all future action is governed by *necessity*. The best forms, or those nerves most sensitive to light, connected with incipient membranes and humors, for corneas and lenses, are picked out and preserved by natural selection, of necessity. All cannot live and propagate, and it is a necessity, obvious to all, that the weaker must perish, if the theory be true. Working on, in this way, through countless generations, the eye is at last formed in all its beauty and excellence. It must, (always assuming that this theory is true,) result from this combined action of natural variation, the struggle for life, and natural selection, with as much certainty as the balls, after collision, must pass to corners of the table different from those to which they were directed, and so far forth, as the eye is formed by these laws, acting upwards from the nerve merely sensitive to light, we can no more infer design, and from design, a designer, than we can infer design in the direction of the billiard balls after collision. Both are sufficiently accounted for by blind powers acting under a blind necessity. Take away the struggle for life from the one, and the collision of the balls from the other,—and neither of these were designed,—and the animal would have gone on without eyes. The balls would have found the corners of the table to which they were first directed.

While, therefore, it seems to me clear that one who can find no proof of the existence of an intelligent creator, except through the evidence of design in the organic world, can find no evidence of such design in the construction of the eye, if it were constructed under the operation of Darwin's laws; I shall not for one moment contend that these laws are *incompatible* with design and a self-conscious, intelligent creator. Such design, might indeed, have coexisted with the necessity or natural selection; and so the billiard players might have designed the collision of their balls; but neither the formation of the eye, nor the path of the balls after collision, furnishes any sufficient proof of such design in either case.

One, indeed, who believes from revelation or any other cause, in the existence of such a Creator, the fountain and source of all things in heaven above and in the earth beneath, will see in natural variation, the struggle for life and natural selection, only the order or mode, in which this Creator, in his own perfect wisdom, sees fit to act. Happy is he who can thus see and adore. But how many are there who have no such belief from intuition, or faith in revelation; but who have by careful and elaborate search in the physical, and more especially in the organic world, inferred, by induction, the existence of God from what has seemed to them the wonderful adaptation of the different organs and parts of the animal body to its, apparently, designed ends! Imagine a mind of this skeptical character, in all honesty and under its best reason, after finding itself obliged to reject the evidence of revelation, to commence a search after the Creator, in the light of natural theology. He goes through the proof for final cause and design, as given in a summary though clear, plain, and convincing form in the pages of Paley, and the Bridgewater treatises. The eye and the hand, those perfect instruments of optical and mechanical contrivance and adaptation, without the least waste or surplusage;—these, say Paley and Bell, certainly prove a designing maker as much as the palace or the watch prove an architect or a watchmaker. Let this mind, in this state, cross Darwin's work, and find that after a sensitive nerve, or a rudimentary hoof or claw, no design is to be found. From this point upwards the development is the mere necessary result of natural selection; and let him receive this law of natural selection as true, and where does he find himself? Before, he could refer the existence of the eye, for example, only to design, or chance. There was no other alternative. He rejected chance, as impossible. It must then be design. But Darwin brings up another power, namely, natural selection, in place of this impossible chance. This not only may, but, according to Darwin, must of necessity produce an eye. It may indeed coexist with design, but it must exist and act and produce its results, even without design. Will such a mind, under such circumstances, infer the existence of the designer—God—when he can, at the same time, satisfactorily account for the thing produced, by the operation of this natural selection? It seems to me, therefore, perfectly evident that the substitution of natural selection, by necessity, for design in the formation of the organic world, is a step decidedly atheistical. It is in vain to say that Darwin takes the creation of organic life, in its simplest forms, to have been the work of the Deity. In giving up design in these highest and most complex forms of organization, which have always been relied upon as the crowning proof of the existence of an intelligent Creator, without whose intellectual power

they could not have been brought into being; he takes a most decided step to banish a belief in the intelligent action of God from the organic world. The lower organisms will go next.

The atheist will say, wait a little. Some future Darwin will show how the simple forms came *necessarily* from inorganic matter. This is but another step by which, according to La Place, 'the discoveries of science throw final causes further back.'

SECOND READER.—It is conceded that if the two players in the supposed case were ignorant of each other's presence the designs of both were frustrated, and from necessity. Thus far it is not needful to inquire whether this necessary consequence is an unconditional or a conditioned necessity, nor to require a more definite statement of the meaning attached to the word *necessity* as a supposed third alternative.

But if the players knew of each other's presence, we could not infer from the result that the design of both or of either was frustrated. One of them may have intended to frustrate the other's design, and to effect his own. Or both may have been equally conversant with the properties of the matter and the relation of the forces concerned, (whatever the cause, origin, or nature of these forces and properties), and the result may have been according to the designs of both.

As you admit that they might or might not have designed the collision of their balls and its consequences, the question arises whether there is any way of ascertaining which of the two conceptions we may form about it, is the true one. Now, let it be remarked that *design* can never be *demonstrated*. Witnessing the act does not make known the *design*, as we have seen in the case assumed for the basis of the argument. The word of the actor is not proof; and that source of evidence is excluded from the cases in question. The only way left, and the only possible way in cases where testimony is out of the question, is to infer the design from the result, or from arrangements which strike us as *adapted* or *intended* to produce a certain result, which affords a presumption of design. The strength of this presumption may be zero, or an even chance, as perhaps it is in the assumed case; but the probability of design will increase with the particularity of the act, the speciality of the arrangement or machinery, and with the number of identical or yet more of similar and analogous instances, until it rises to a moral certainty,—*i. e.*, to a conviction which practically we are as unable to resist as we are to deny the cogency of a mathematical demonstration. A single instance, or set of instances, of a comparatively simple arrangement might suffice. For instance, we should not doubt that a pump was designed to raise water by the moving of the handle. Of course the conviction is the stronger, or at least the sooner

arrived at, where we can imitate the arrangement, and ourselves produce the result at will, as we could with the pump, and also with the billiard-balls.

And here I would suggest that your billiard-table with the case of collision, answers well to a machine. In both, a result is produced by indirection,—by applying a force out of line of the ultimate direction. And, as I should feel as confident that a man intended to raise water who was working a pump-handle, as if he was bringing it up in pails-full from below by means of a ladder, so, after due examination of the billiard-table and its appurtenances, I should probably think it likely that the effect of the rebound was expected and intended no less than that of the immediate impulse. And a similar inspection of arrangements and results in nature would raise at least an equal presumption of design.

You allow that the rebound might have been intended, but you require proof that it was. We agree that a single such instance affords no evidence either way. But how would it be if you saw the men doing the same thing over and over? and if they varied it by other arrangements of the balls or of the blow, and these were followed by analogous results? How if you at length discovered a profitable end of the operation, say the winning of a wager? So in the counterpart case of natural selection; must we not infer intention from the arrangements and the results? But I will take another case of the very same sort, though simpler, and better adapted to illustrate natural selection; because the change of direction,—your necessity—acts gradually or successively, instead of abruptly.

Suppose I hit a man standing obliquely in my rear, by throwing forward a crooked stick, called a boomerang. How could he know whether the blow was intentional or not? But suppose I had been known to throw boomerangs before; suppose that, on different occasions, I had before wounded persons by the same, or other indirect and apparently aimless actions; and suppose that an object appeared to be gained in the result, *i. e.*, that definite ends were attained—would it not at length be inferred that my assault, though indirect, or apparently indirect, was designed?

To make the case more nearly parallel with those it is brought to illustrate, you have only to suppose that, although the boomerang thrown by me went forward to a definite place, and at least appeared to subserve a purpose, and the bystanders, after a while, could get traces of the mode or the empirical law of its flight, yet they could not themselves do anything with it. It was quite beyond their power to use it. Would they doubt, or deny *my* intention, on that account? No: they would insist that design on my part must be presumed from the nature of the results;—that, though design *may* have been wanting in any one

case, yet the repetition of the result, and from different positions and under varied circumstances, showed that there *must* have been design.

Moreover, in the way your case is stated, it seems to concede the most important half of the question, and so affords a presumption for the rest, on the side of design. For you seem to assume an actor, a designer, accomplishing his design in the first instance. You—a bystander—infer that the player effected his design in sending the first ball to the pocket before him. You infer this from observation alone. Must you not from a continuance of the same observation equally infer a common design of the two players in the complex result, or a design of one of them to frustrate the design of the other? If you grant a designing actor, the presumption of design is as strong, or upon continued observation of instances soon becomes as strong, in regard to the deflection of the balls, or variation of the species, as it was for the result of the first impulse or for the production of the original animal, &c.

But in the case to be illustrated, we do not see the player. We see only the movement of the balls. Now, if the contrivances and adaptations referred to (p. 229,) really do "prove a designer as much as the palace or the watch prove an architect or a watchmaker,"—as Paley and Bell argue, and as your skeptic admits, while the alternative is between design and chance,—then they prove it with all the proof the case is susceptible of, and with complete conviction. For we cannot doubt that the watch had a watchmaker. And if they prove it on the supposition that the unseen operator acted *immediately*,—*i. e.*, that the player directly impelled the balls in the directions we see them moving, I insist that this proof is not impaired by our ascertaining that he acted *mediately*, *i. e.*, that the present state or form of the plants or animals, like the present position of the billiard-balls, resulted from the collision of the individuals with one another, or with the surroundings. The original impulse, which we supposed was in the line of the observed movement, only proves to have been in a different direction; but the series of movements took place with a series of results, each and all of them none the less determined, none the less designed.

Wherefore, when, at the close, you quote Laplace, that "the discoveries of science throw final causes farther back," the most you can mean is, that they constrain us to look farther back for the impulse. They do not at all throw *the argument for design* farther back, in the sense of furnishing evidence or presumption that only the primary impulse was designed, and that all the rest followed from chance or necessity.

Evidence of design, I think you will allow, every where is drawn from the observation of adaptations and of results, and

has really nothing to do with any thing else, except where you can take the *word* for the *will*. And in that case you have not *argument for design*, but *testimony*. In nature we have no testimony; but the argument is overwhelming.

Now, note that the argument of the olden time,—that of Paley, &c., which your skeptic found so convincing,—was always the argument for design in the movement of the balls *after deflection*. For it was drawn from animals produced by generation, not by creation, and through a long succession of generations or deflections. Wherefore, if the argument for design is perfect in the case of an animal derived from a long succession of individuals as nearly alike as offspring is generally like parents and grand-parents, and if this argument is not weakened when a variation, or series of variations, has occurred in the course, as great as any variations we know of among domestic cattle, how then is it weakened by the supposition, or by the likelihood, that the variations have been twice or thrice as great as we formerly supposed, or because the variations have been 'picked out,' and a few of them preserved as breeders of still other variations, by natural selection?

Finally let it be noted that your element of *necessity*, has to do, so far as we know, only with the picking out and preserving of certain changing forms, *i. e.*, with the natural selection. This selection, you may say, must happen under the circumstances. This is a necessary result of the collision of the balls; and these results can be predicted. If the balls strike so and so, they will be deflected so and so. But the *variation* itself is of the nature of an origination. It answers well to the original impulse of the balls, or to a series of such impulses. We cannot predict what particular new variation will occur from any observation of the past. Just as the first impulse was given to the balls at a point out of sight, so the impulse which resulted in the variety or new form was given at a point beyond observation, and is equally mysterious or unaccountable, except on the supposition of an ordaining will. The parent had not the peculiarity of the variety, the progeny has. Between the two is the dim or obscure region of the formation of a new individual, in some unknown part of which, and in some wholly unknown way, the difference is intercalated. To introduce necessity here is gratuitous and unscientific; but here you must have it to make your argument valid.

I agree that judging from the past—it is not improbable that variation itself may be hereafter shown to result from physical causes. When it is so shown you may extend your necessity into this region, but not till then. But the whole course of scientific discovery goes to assure us that the discovery of the cause of variation will be only a resolution of variation into two

factors,—one, the immediate secondary cause of the changes, which so far explains them; the other an unresolved or unexplained phenomenon, which will then stand just where the product, variation, stands now, only that it will be one step nearer to the efficient cause.

This line of argument appears to me so convincing, that I am bound to suppose that it does not meet your case. Although you introduced players to illustrate what design is, it is probable that you did not intend, and would not accept, the parallel which your supposed case suggested. When you say that the proof of design in the eye and the hand, as given by Paley and Bell, was convincing, you mean, of course, that it was convincing, so long as the question was between *design* and *chance*, but that now another alternative is offered, one which obviates the force of those arguments, and may account for the actual results without design. I do not clearly apprehend this third alternative.

Will you be so good, then, as to state the grounds upon which you conclude that the supposed proof of design from the eye, or the hand, as it stood before Darwin's theory was promulgated, would be invalidated by the admission of this new theory.

First Reader.—As I have ever found you, in controversy, meeting the array of your opponent, fairly and directly, without any attempt to strike the body of his argument through an unguarded joint in the phraseology, I was somewhat surprised at the course taken in your answer to my statement on Darwin's theory. You there seem to suppose that I instanced the action of the billiard balls and players as a parallel, throughout, to the formation of the organic world. Had it occurred to me that such an application might be supposed to follow, legitimately, from my introduction of this action, I should certainly have stated that I did not intend, and should by no means accede to, that construction. My purpose in bringing the billiard table upon the scene was to illustrate, by example, *design* and *necessity*, as different and independent sources from which results, it might indeed be identical results, may be derived. All the conclusions therefore that you have arrived at through this misconception or misapplication of my illustration, I cannot take as an answer to the matter stated or intended to be stated by me. Again, following this misconception, you suppose the skeptic (instanced by me as revealing through the evidence of design, exhibited in the structure of the eye, for its designer, God,) as bringing to the examination a belief in the existence of design in the construction of the animals as they existed up to the moment when the eye was, according to my supposition, added to the heart, stomach, brain, &c. By skeptic I, of course, intended one who doubted the existence of design in every organic struc-

ture, or at least required proof of such design. Now as the watch may be instanced as a more complete exhibition of design than a flint knife or an hour-glass; I selected, after the example of Paley, the eye, as exhibiting by its complex but harmonious arrangements a higher evidence of design and the designer, than is to be found in a nerve sensitive to light, or any mere rudimentary part or organ. I could not mean by skeptic one who believed in design so far as a claw, or a nerve sensitive to light was concerned, but doubted all above. For one who believes in design at all will not fail to recognize it in a hand or an eye. But I need not extend these remarks, as you acknowledge in the sequel to your argument that you may not have have suited it to the case as I had stated it.

You now request me to "state the grounds upon which I conclude that the supposed proof of design from the eye and the hand, as it stood before Darwin's theory was promulgated, is invalidated by the admission of that theory." It seems to me that a sufficient answer to this question has already been made in the last part of my former paper; but as you request it I will go over the leading points as there given with more minuteness of detail.

Let us then suppose a skeptic, one who is yet considering and doubting of the existence of God, having already concluded that the testimony from any and all revelation is insufficient, and having rejected what is called the *a priori* arguments brought forward in natural theology, and pertinaciously insisted upon by Dr. Clark and others, turning as a last resource to the argument from design in the organic world. Voltaire tells him that a palace could not exist without an architect to design it. Dr. Paley tells him that a watch proves the design of a watchmaker. He thinks this very reasonable, and although he sees a difference between the works of nature and those of mere human art, yet if he can find in any organic body, or part of a body, the same adaptation to its use that he finds in a watch, this truth will go very far towards proving, if it is not entirely conclusive, that in making it, the powers of life by which it grew were directed by an intelligent, reasoning master. Under the guidance of Paley he takes an eye, which, although an optical, and not a mechanical, instrument like the watch, is as well adapted to testify to design. He sees, first that the eye is transparent, when every other part of the body is opaque. Was this the result of a mere Epicurean or Lucretian "fortuitous concourse" of living "atoms?" He is not yet certain it might not be so. Next he sees that it is spherical and that this convex form alone is capable of changing the direction of the light which proceeds from a distant body, and of collecting it so as to form a distinct image within its globe. Next he sees at the exact place where this image must

be formed a curtain of nerve work, ready to receive and convey it, or excite from it, in its own mysterious way, an idea of it in the mind. Last of all, he comes to the crystalline lens. Now he has before learned that without this lens an eye would by the aqueous and vitreous humors alone form an image upon the retina, but this image would be indistinct from the light not being sufficiently refracted, and likewise from having a colored fringe round its edges. This last effect is attributable to the refrangibility of light, that is, to some of the colors being more refracted than others. He likewise knows that more than a hundred years ago Mr. Dollond having found out, after many experiments, that some kinds of glass have the power of dispersing light, for each degree of its refraction, much more than other kinds, and that on the discovery of this fact, he contrived to make telescopes in which he passed the light through two object-glasses successively, one of which he made of crown and one of flint glass, so ground and adapted to each other that the greater dispersion produced by the substance of one should be corrected by the smaller dispersion of the other. This contrivance corrected entirely the colored images which had rendered all previous telescopes very imperfect. He finds in this invention all the elements of design, as it appeared in the thought and action of a human designer. First, conjecture of certain laws or facts in optics. Then, experiment proving these laws or facts. Then, the contrivance and formation of an instrument by which those laws or facts must produce a certain, sought, result.

Thus enlightened, our skeptic turns to his crystalline lens to see if he can discover the work of a Dollond in this. Here he finds that an eye, having a crystalline lens placed between the humors, not only refracts the light more than it would be refracted by the humors alone, but that in this combination of humors and lens, the colors are as completly corrected as in the combination of Dollond's telescope. Can it be that there was no design, no designer, directing the powers of life in the formation of this wonderful organ? Our skeptic is aware that in the arts of man, great aid has been, sometimes, given by chance, that is, by the artist or workman observing some fortuitous combination, form, or action around him. He has heard it said that the chance arrangement of two pairs of spectacles, in the shop of a Dutch optician, gave the direction for constructing the first telescope. Possibly, in time, say a few geological ages, it might in some optician's shop, have brought about a combination of flint and crown glass which, together, should have been achromatic. But the space between the humors of the eye is not an optician's shop where object-glasses of all kinds, shapes, and sizes are placed by chance, in all manner of relations and positions. On the hypothesis under which our skeptic is making his examina-

tion,—the eye having been completed in all but the formation of the lens,—the place which the lens occupies when completed, was filled with parts of the humors and plane membrane, homogeneous in texture and surface, presenting, therefore, neither the variety of the materials, nor forms which are contained in the optician's shop for chance to make its combinations with. How then could it be cast of a combination not before used, and fashioned to a shape different from that before known, and placed in exact combination with all the parts before enumerated, with many others not even mentioned? He sees no parallelism of condition then, by which chance could act in forming a crystalline lens, which answers to the condition of an optician's shop, where it might be possible in many ages for chance to combine existing forms into an achromatic object-glass.

Considering, therefore, the eye thus completed and placed in in its bony case and provided with its muscles, its lids, its tear-ducts, and all its other elaborate and curious appendages, and, a thousand times more wonderful still, without being encumbered with a single superfluous or useless part, can he say that this could be the work of chance? The improbability of this is so great, and consequently the evidence of design is so strong, that he is about to seal his verdict in favor of design when he opens Mr. Darwin's book.

There he finds that an eye is no more than a vital aggregation or growth, directed, not by design nor chance, but moulded by natural variation and natural selection, through which it must, necessarily, have been developed and formed. Particles or atoms being aggregated by the blind powers of life, must become under the given conditions, by natural variation and natural selection, eyes, without design, as certainly as the red billiard ball went to the west pocket, by the powers of inertia and elasticity, without the design of the hand that put in motion. (See Darwin, p. 169.)

Let us lay before our skeptic the way in which we may suppose that Darwin would trace the operation of life, or the vital force conforming to these laws. In doing this we need not go through with the formation of the several membranes, humors, &c., but take the crystalline lens as the most curious and nicely arranged and adapted of all the parts, and as giving moreover a close parallel, in the end produced, to that produced by design, by a human designer, Dollond, in forming his achromatic object-glass. If it can be shown that natural variation and natural selection were capable of forming the crystalline lens, it will not be denied that they were capable of forming the iris, the sclerotica, the aqueous humors, or any and all the other parts. Suppose, then, that we have a number of animals, with eyes yet wanting the crystalline. In this state the animals can see, but

dimly and imperfectly, as a man sees after having been couched. Some of the offspring of these animals have, by *natural variation*, merely, a portion of the membrane which separates the aqueous from the vitreous humor, a little thickened in its middle part, a little swelled out. This refracts the light a little more than it would be refracted by a membrane in which no such swelling existed, and not only so, but in combination with the humors, it corrects the errors of dispersion and makes the image somewhat more colorless. All the young animals that have this swelled membrane see more distinctly than their parents or brethren. They, therefore, have an advantage over them in the *struggle for life*. They can obtain food more easily; can find their prey, and escape from their enemies with greater facility than their kindred. This thickening and rounding of the membrane goes on from generation to generation by natural variation; natural selection all the while "picking out with unerring skill all the improvements, through countless generations," until at length it is found that the membrane has become a perfect crystalline lens. Now where is the design in all this? The membrane was not thickened and rounded to the end that the image should be more distinct and colorless; but, being thickened and rounded by the operation of natural variation, *inherent* in generation, natural selection *of necessity* produced the result that we have seen. The same result was thus produced *of necessity*, in the eye, that Dollond came at, in the telescope, with design, through painful guessing, reasoning, experimenting, and forming.

Suppose our skeptic to believe in all this power of natural selection; will he now seal up his verdict for design, with the same confidence that he would before he heard of Darwin? If not, then "the supposed proof from design is invalidated by Darwin's theory."

Second Reader.—Waiving incidental points and looking only to the gist of the question, I remark that, the argument for design as against chance in the formation of the eye, is most convincingly stated by you on p. 235-237. Upon this and numerous similar arguments the whole question we are arguing turns. So, if the skeptic was about to seal his verdict in favor of design, and a designer, when Darwin's book appeared, why should his verdict now be changed or withheld? All the facts about the eye, which convinced him that the organ was designed, remain just as they were. His conviction was not produced through testimony or eye-witness, but design was irresistibly inferred from the evidence of contrivance in the eye itself.

Now, if the eye as it is, or has become, so convincingly argued design, why not each particular step or part of this result?

If the production of a perfect crystalline lens in the eye—you know not how,—as much indicated design, as did the production of a Dollond achromatic lens,—you understand how—then why does not "the swelling out" of a particular portion of the membrane behind the iris—caused you know not how—which, by "correcting the errors of dispersion and making the image somewhat more colorless," enabled the "young animals to see more distinctly than their parents or brethren," equally indicate design —if not as much as a perfect crystalline, or a Dollond compound lens, yet as much as a common spectacle glass?

Darwin only assures you that what you may have thought was done directly and at once, was done indirectly and successively. But you freely admit that indirection and succession do not invalidate design, and also that Paley and all the natural theologians drew the arguments which convinced your skeptic wholly from eyes indirectly or naturally produced.

Recall a woman of a past generation and show her a web of cloth; ask her how it was made, and she will say that the wool or cotton was carded, spun, and woven by hand. When you tell her it was not made by manual labor, that probably no hand has touched the materials throughout the process, it is possible that she might at first regard your statement as tantamount to the assertion that the cloth was made without design. If she did, she would not credit your statement. If you patiently explained to her the theory of carding machines, spinning jennys, and power-looms, would her reception of your explanation weaken her conviction that the cloth was the result of design? It is certain that she would believe in design as firmly as before, and that this belief would be attended by a higher conception and reverent admiration of a wisdom, skill, and power so greatly beyond any thing she had previously conceived possible.

Wherefore, we may insist that, for all that yet appears, the argument for design, as presented by the natural theologians, is just as good now, if we accept Darwin's theory, as it was before that theory was promulgated; and that the skeptical Juryman, who was about to join the other eleven in an unanimous verdict in favor of design, finds no good excuse for keeping the Court longer waiting.

ART. XXIII.—*Description of three New Meteoric Irons, from Nelson County, Ky., Marshall County, Ky., and Madison County, North Carolina;* by J. LAWRENCE SMITH, M.D., Prof. of Chemistry, University of Louisville, Ky.

Nelson County, (Ky.) Meteorite.—This came into my possession about two months ago, being obtained from a ploughed field where it may have laid for a considerable length of time, attention was drawn to it by a plough striking it; its metallic character leading the neighboring farmer to believe it to be silver.

It is a flattened mass of tough metal, a little scaly at one corner, being 17 inches long, 15 inches broad, and 7 inches in the thickest part, shelving off like the back of a turtle, and weighs 161 lbs.

It is free from any large proportion of thick rust, consequently showing no indications of chlorine. On analysis, the following constituents were found in 100 parts, No. 1 in the table below:

	(1.)	(2.)	(3.)
Iron,	93·10	90·12	91·12
Nickel,	6·11	8·72	7·82
Cobalt,	·41	·32	·43
Phosphorus,	·05	·10	·08
Copper,	*trace*	*trace*	*trace*
	99·67	99·26	99·45

Marshall County, (Ky.) Meteorite.—A piece of this Meteorite was sent to me from Marshall County, in this State. I have not yet seen the entire mass, which is said to weigh 15 lbs., and to be scaly in structure. It has the usual characteristics of meteoric iron, as seen from the analysis, No. 2.

Madison County, (N. C.) Meteorite.—This meteorite was presented to me some time ago by the Hon. T. L. Clingman, of North Carolina. It came from Jewel Hill, Madison County, of that State. There is a great deal of thick rust on the surface, with constant deliquescence from chlorid of iron. Its form and surface indicates that it is entire, its dimensions are $7\times6\times3$ inches, with a number of indentations; its weight is 8 lb. 13 oz. Its composition is given in the analysis, No. 3.

ART. XXIV.—*Description of a new Trilobite from the Potsdam Sandstone;* by FRANK H. BRADLEY, with a note by E. BILLINGS.

[Read before the Am. Assoc. for the Advancement of Science, at Newport.]

CONOCEPHALITES MINUTUS, (n. sp.)

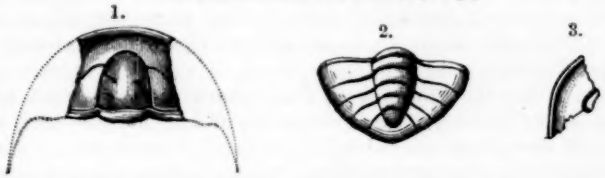


Fig. 1. The head magnified. The dotted lines represent the supposed outlines of the parts not preserved.
Fig. 2. The pygidium magnified.
Fig. 3. A detached cheek, magnified.

Cephalic shield apparently semi-circular, or nearly so; anterior margin as far as preserved with a narrow slightly elevated rim, just within which there is a rather strong groove. Glabella conical, slightly narrowed at the neck segment, three-fourths the whole length of the head, very convex and obtusely carinated along the median line. Neck segment rounded and prominent; neck furrow narrow, but well defined. There are two pairs of deep glabellar furrows which are inclined inwards and backwards at an angle of about 45°; their inner extremities distant from each other rather more than one-third the width of the glabella. The anterior lobe is a little less than one-half the whole length of the glabella, excluding the neck segment; the two posterior pairs are nearly equal to each other. The glabella is distinctly separated from the cheeks by a narrow, deep groove, which extends all round. From the anterior lobe on each side a narrow filiform ridge curves outwards and backwards on the fixed cheek to the edge of the portion preserved. The eyes appear to be situated just where these ridges terminate as represented in figure 1. Judging from the portion of the eye preserved in a detached cheek-plate, its form is semi-annular, and its length at least one-fourth that of the glabella. Its distance must be at least one-half the width of the glabella. Caudal shield nearly as large as the head, its width scarcely equal to half its length; the lobes nearly equal; the middle lobe very convex with five sharp transverse grooves; the side lobes somewhat flat, and each with five grooves.

The largest head discovered is exactly two lines in length.

The course of the facial suture has not been ascertained. The surface of the glabella in one of the specimens appears to be smooth, but in none of the others can it be distinguished.

The specimens are mostly in a clayey layer, which is full of fragments of all degrees of perfection; in one specimen I count ten heads and three tails, all in a fair state of preservation. In two instances, I have found the casts of maxiliary plates, showing distinctly the elevated margin, of one of which I give a figure.

The original specimens were collected, (at High Bridge, near Keeseville, N. Y.,) in August, 1856, while on a geological excursion with Col. Jewett of Albany, but were not recognized until July, 1857, when a second visit to the locality secured a few *casts* in the solid sandstone, none of the clay layer being obtained. By the kindness of Prof. Dana, the specimens were presented at the Am. Assoc. for Adv. of Sci. at Montreal, but were not recognized as belonging to any known species. Since that time, I have looked for descriptions, but cannot find any to correspond.

At the same locality, I also procured the cast, a Pleurotomaria, and one of what seems to have been a plate from the stem of a crinoid.

New Haven, June 15th, 1860.

Note by E. Billings.—Mr. Bradley having favored me with a view of his very interesting specimens, I think there can be no doubt but that they belong to the genus *Conocephalites*. If this reference be correct, then we have at least three, if not four species in North America.

1. *C. antiquatus* (Salter,) described from "a cast in a brown sandstone, said to be a bouldered fragment from Georgia." (See Quart. Jour. Geol. Soc., vol. xv, p. 554.)

2. *C. minutus* (Bradley.) In this species, the form of the glabella and its proportions in relation to the length of the head are almost precisely the same as in *C. antiquatus*, and yet I think the two are not identical, for the following reasons: In the first place, all the specimens of *C. minutus* are of a nearly uniform size, the length of the head being about two lines, and, therefore, it seems probable that they are the remains of adult individuals. The total length would thus be about half an inch, while Mr. Salter's species is full one inch and three-fourths. In the second place, the distance of the eye from the glabella, in *C. antiquatus* is only one-third the width of the glabella, but in *C. minutus* it must be at least one-half the width. These are the only differences that can be well made out, from the imperfect specimens, but they seem to me sufficient to indicate two species. Mr. Salter says further, that the lobes of the glabella in *C. antiquatus* are very obscure, and that the ocular ridge, if any existed, must have been very slight. His specimen was somewhat abraded. In *C. minutus* the ocular ridge is, for so small a species, very strongly defined, and the glabellar furrows are so deep

that it would require a very considerable amount of abrasion to obliterate them.

3. *C. Zenkeri,* (n. sp.) This is a new species recently discovered in the magnesian limestone near Quebec. It will probably be described in the next No. of the Canadian Naturalist and Geologist.

4. There is in the collection of the geological survey of Canada, a plaster cast of the surface of a fragment of rock which holds four specimens of a trilobite, each about the size of *C. antiquatus.* They appear to me to belong to the genus *Conocephalites.* The original specimen was collected in Newfoundland, in the same slate that holds *Paradoxides Bennettii* (Salter,) and I am informed that it is in the possession of a gentleman who lives somewhere in the United States, but whose name or place of residence, I have not been able to ascertain.

Of the above four species, Mr. Bradley's is at present the most important as it fixes indisputably, at least one point in the geological range of the genus on this side of the Atlantic. In Europe, *Conocephalites* has not been found out of the *primordial zone* of Barrande, but the Quebec and Keeseville specimens show that here it reaches the Lower Silurian.

Montreal, July 25th, 1860.

ART. XXV.—*On the Combustion of Wet Fuel, in the Furnace of Moses Thompson;* by B. SILLIMAN, Jr., Prof. Gen. and App. Chem. in Yale College.

[Read before the Am. Assoc. for the Adv. of Sci., at Newport, August, 1860.]

IN all ordinary modes of combustion, it is well known that the use of wet fuel is attended with a very great loss of heat, rendered latent in the conversion of water into steam. As the most perfectly air dried wood still contains about 25 per centum of water, according to the experiments of Rumford, the term *wet fuel* might seem appropriate to all fuels, but mineral coal and charcoal. But technically, this term is restricted to substances like peat and those residual products of the arts which, like spent tan, begasse and dye stuffs contain at least one half and often more than half of their weight of water. Until a recent period the attempt to consume these products as sources of heat has been attended with uneconomical results, or total failure. It is the object of this paper to describe a mode of combustion in which by a modification in the form of the furnace the combustion of wet fuel is not only rendered consistent with the best economical results; but which as it involves chemical reactions never before, it is believed, successfully applied for such

purposes, is deserving of particular notice from a scientific as well as from a practical point of view.

It is a well established fact in chemistry, that the affinity of carbon for oxygen, at high temperatures is so strong, that if oxygen is not present in a free state, any compound containing oxygen, which happens to be present is decomposed, in order to satisfy this affinity. This fact is well illustrated in the familiar case of the Blast Furnace where this affinity is employed to deprive the ores of iron of their oxygen in the process of reduction to metallic iron.

In the first stages of combustion, in wet fuels, the chief products given off are steam from the drying of the wet mass, smoke or volatilized carbon and oxyd of carbon, with, of course, a variable proportion of carbonic acid and carburetted hydrogen. These products in all ordinary furnaces, pass on together into the stack, carrying with them the heat which they have absorbed and rendered latent. The problem presented is then to recover the heat thus locked up and lost, and by the furnace now under consideration *this is accomplished by shutting off almost entirely the access of the outer air and causing the wet fuel to supply its own supporter of combustion drawn from the decomposition of the vapor of water at a high temperature by its reaction with free carbon and the oxyd of carbon.*

The practical solution of this problem was first successfully accomplished, as appears from a decision of Patent Commissioner Holt, by the late Moses Thompson, in 1854. The controversial questions growing out of this invention, are entirely foreign to our present purpose and in no way affect its practical or scientific value. Suffice it to say, in passing, that we find in this invention another instance of a truth already so often signalized in the history of inventions, that important results are often obtained, of the highest value in promoting material prosperity and the welfare of society, by those who are guided in their search only by the result in view, and not by any exact knowledge of the scientific principles involved.

Mr. Thompson seems to have been inspired with the conviction that if he could bring the products from the combustion of wet fuel together in a place, hot enough for the purpose, and from which the atmospheric air was excluded, they would, as he expresses it in his patent, mutually "*consume each other.*" This notion was realized, and the reaction secured between the elements of water and the carbon of smoke, or the oxyd of carbon in a part of the furnace called by the inventor, the *mixing chamber.*

Wherever that place may be situated, or however constructed, the one essential thing about it, is, that it should be a very hot place, and one to which the atmospheric air can have no direct access, until it has passed by, and through the burning fuel. It

is in fact a retort or place for combination and reaction, and may be a distinct chamber or flue, or only a recess or enlargement greater or less of the main furnace. Wherever it may be placed, or however built, it must meet the essential conditions of a high temperature, and of atmospheric isolation. In this mixing chamber, then, the important chemical reaction before insisted on, must be set up. The vapor of water is decomposed, furnishing its oxygen to the highly heated carbon to form carbonic acid, while the oxyd of carbon is in like manner exalted to the same condition, and any excess of carbon forms with free hydrogen, marsh gas or light carburetted hydrogen. The vapor of water is thus made to give up not only its constituent elements to form new compounds with oxygen, producing in the change great heat, but a great part of the heat absorbed by the water in becoming steam is also liberated in this change of its physical and chemical condition. Moreover as all these products of combustion and of chemical reaction pass together over the bridge-wall of the furnace into a space from which atmospheric air is not excluded, it then and there happens that any free hydrogen, light carburetted hydrogen or oxyd of carbon which have previously escaped combustion, take fire and burn, yielding up their quota of heat to the general aggregate.

Such is the intensity of heat in that portion of the furnace where these reactions take place that only the most solid structures of refractory fire bricks will endure it, and the color seen throughout that portion of the furnace is of the purest white.

In view of the facts already stated it is easy to understand why it is that when the reactions described are once set up, the admission of a free current of atmospheric air should immediately check the energy of the combustion and soon result in total suspension of the peculiar energy of this furnace. The air containing only one-fifth part of its bulk of oxygen gas, the active agent in combustion, the access of so large a proportion of cold air—four-fifths of which are not only indifferent but positively prejudicial from the quantity of heat it absorbs,—it happens that the temperature of the mixing chamber is rapidly reduced below the point at which carbon can decompose vapor of water and the instant that point is reached the arrival of fresh supplies of steam completes the decline of energy and the furnace commences forthwith to belch forth from its stack dense volumes of smoke and watery vapor. When in proper action not a particle of smoke is visible from the stack of a furnace in which wet fuel is burning, and what is more remarkable the reactions are so evenly balanced that no wreaths of watery vapor are observed, while in the earlier stages of combustion before the proper temperature in the mixing chamber is reached, both these products are seen in great abundance.

DESCRIPTION OF THE FURNACES.

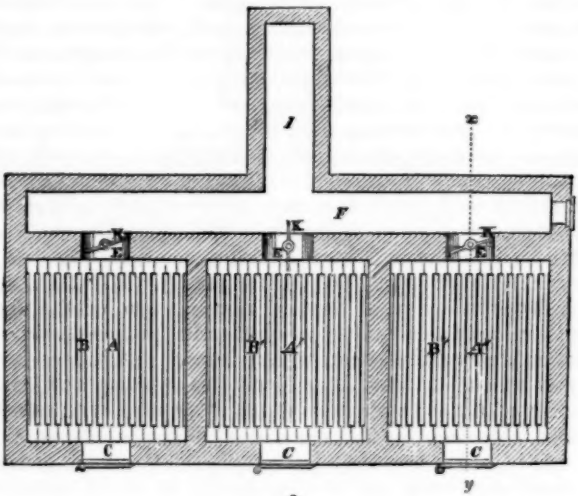
1st. Furnace for combustion of wet tan, sawdust, &c.

Fig. 1, is a horizontal section of a furnace constructed according to the specifications of Thompson's first patent, (issued April 10th, 1855).

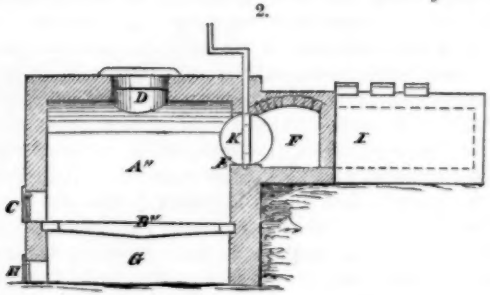
Fig. 2, is a vertical section of the same in the line $x\ y$, of figure 1.

Similar letters indicate corresponding parts in both figures.

1.



2.



The furnace shown in these figures has three square or oblong fire chambers, A, A', A'', side by side, experience having shown that not less than three compartments are required to secure the best results in the practical working of the furnace, although in some cases two may suffice, but frequently more than three are

desirable. The fire chambers are furnished with grate bottoms *B*, *B'*, *B''*, of fire bricks, and are arched at top. Each chamber has a door *C*, in front for lighting and tending the fire. This opening is seldom used after the furnace is once set in action. The wet fuel is supplied through the opening *D* at top. *E* is an opening at the back of each chamber leading to the flue *F*, or the mixing chamber. This opening may be provided with a damper *K* (which must be of fire clay, if of iron the intense heat soon destroys it). Each chamber has a separate ash pit *G* with its opening *H*. This although called an ash pit serves a most important purpose in the economy of the furnace as a receptacle for the burning coals which constantly fall into it from the lower part of the wet mass above, as will be more particularly explained beyond.

If the furnace is used for generating steam the best place for the boiler is over the flue *I*. The inventor remarks in his first patent that the current from the mixing chamber in passing to the place of use, in the case of burning wet tan or other very wet fuel, should descend or pass under a bridge to the place of use equal to about one half of the depth of the burning chamber between the grate and the crown, then rise to the place of use. In case of dry or nearly dry fuel, such as green wood and saw dust, the current should rise immediately after leaving the burning chamber to the place of use.

The mode of conducting the operation of the furnace is as follows: fires being lighted in all the fire chambers with dry fuel and the masonry heated to a high degree, two of the three chambers *A A'* are fed with wet fuel and have their ash pits closed. The other fire chamber is kept in action by dry fuel (its ash pit door being proportionally open) until the process of combustion sets in over the surface of the pile of wet fuel resting on the grates of the other furnaces. As soon as this is the case, wet fuel is added by degrees to the third fire chamber, the ash pit door being at the same time closed. If things have been properly managed so far, the process will now continue by the addition of new portions of wet fuel to each furnace in succession or alternately. The temperature of the mixing chamber *F* is now seen to be of the most perfect whiteness and not a visible particle of smoke issues from the stack.

Before discussing this process more in detail, let us first consider the Inventor's description of his furnace as designed more particularly for the consumption of *begasse* or crushed cane stalks.

2. *Furnace for Combustion of Wet Cane Begasse.*

Fig. 3 is a sectional side view, the interior and exterior form of the furnace, and its several parts according to the specifications of Thompson's patent of Dec. 15, 1857.

3.

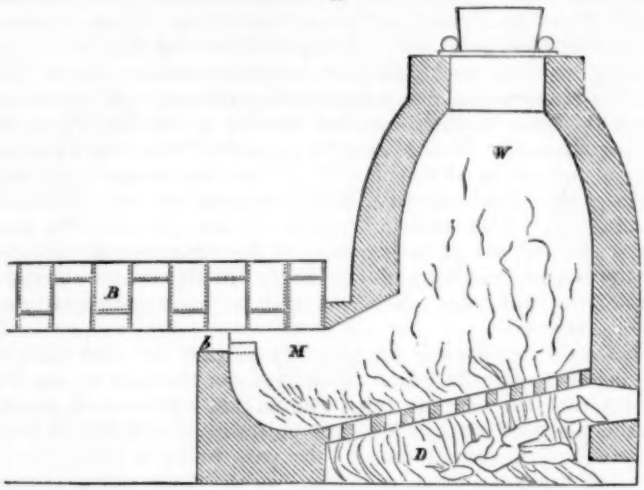


Fig. 4 is a front sectional view of the same, showing the combination of two double furnaces.

4.

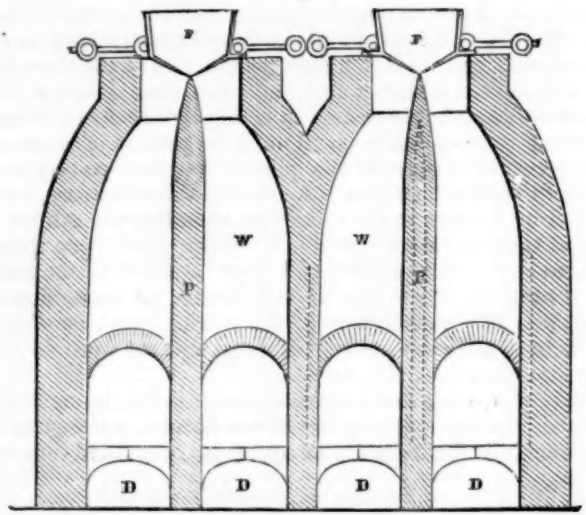
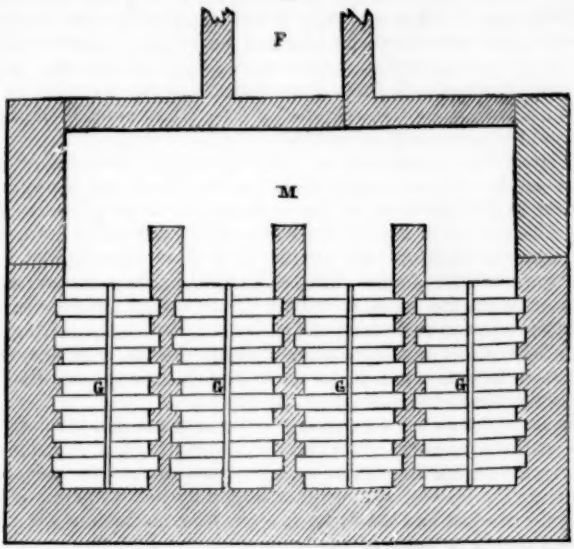


Fig. 5 is a horizontal view of the grate and its relation to the mixing chamber *M* and flue *F*.

5.



Here let the Inventor speak for himself in the language of the patent last named.

"I build two furnaces side by side, each nearly square in its horizontal section. Towards the top I draw in the wall in such manner as to form a kind of dome with a sufficient opening at top to feed the bagasse. The outer walls of these furnaces should be from 24 to 30 inches thick and built with a special view to rendering them non-conducting, the wall near the top, and the partition between the two furnaces may be thinner. In each furnace chamber there should be a partition of fire brick extending across it from front to back and rising nearly to the top, dividing it into two nearly equal parts. The whole interior of the furnace should be of fire brick. The main chamber of each furnace should be divided into two parts—upper and lower—by a fire brick grate about one-fifth the height of the furnace above the hearth, the back end of the grate being a little lower than the front. The bottom of the lower chamber may be a grate with an ash pit, but a hearth is much better.

In each furnace at the front, on each side of the central partition and immediately under the front end of the grate should be doors for feeding wood or other dry fuel, and directly under these doors at the hearth of the lower chamber should be draught openings capable of adjustment to support combustion in the lower chamber.

Extending across the back of both furnaces, and opening into both by flues is a mixing chamber into which all the gases from both furnaces enter in a highly heated state and mix and consume each other on their way to the boiler and stack. This chamber should be about one-half the

capacity of all the fire chambers and it should extend down about as low as the back end of the grate. The flue through which the products of combustion pass out of this chamber and under the boiler should be in section about one square foot to forty cubic feet of mixing chamber.

The feed openings at the top of the furnaces should be closed by doors which open inwards by the weight of the feed, but are self-closing, and do not yield to pressure from within.

The sides of the interior of the upper or wet fuel chamber or drying chamber of the furnace, except the front and back, are corrugated up and down, as also the sides of the central walls or partitions as shown by the dotted lines in Fig. 4, the corrugations extending down to the grate; these corrugations are for the purpose of allowing the heat to radiate upwards from the fire chamber for heating the masonry, and the wet charge, while the gases or vapors driven out of the wet charge by the heat are allowed to descend to the fire chamber or the mixing chamber. If the surfaces of this masonry were smooth the bagasse would lie against them in such a manner as to obstruct the upward radiation of the heat and the downward passage of the vapors.

These corrugations are unnecessary in burning tan and sawdust.

The spaces between the grate bars for burning bagasse should be about 6 inches wide for the finest grinding and twenty inches for the coarsest, and should vary between these widths according to the fineness of grinding, but for sawdust and tan much less, say from one inch to $\frac{3}{4}$ of an inch. The grate should be made of fire brick.

The operation of my furnace is as follows: A hot fire of dry fuel is kindled in the lower or fire chambers of the furnaces and after it has been continued till the masonry is well heated, the chamber above the grate is fed with the begasse or other wet fuel. This hot fire in the fire chamber, especially towards the front of it under the principal mass of the wet fuel, must be preserved throughout the operation. The heat from the masonry and the fire chamber will be communicated to the wet fuel which will cause steam and other gases to issue from it and mix with the intensely hot gases of combustion from the fire chamber, and in a short time the mixing chamber will present intense combustion and heat, the dampers of the fire chambers being partially closed. The lower part of the wet charge will by degrees become dry and charred and will fall through the grate prepared as above unto the fire chamber and supply or nearly supply the place of other dry fuel in preserving the fire in this chamber and the wet fuel being from time to time supplied will furnish in a highly heated state aqueous vapors which descending through the corrugations and otherwise into the fire chamber and mixing chamber, will be decomposed, furnishing much oxygen to the fire, and supply the oxygen necessary to combustion of all the combustible gases issuing from the fire chamber. If by accident the fire in the lower part of the furnace should predominate, the draught should be diminished and more wet fuel added, and, if by accident, the fire in the fire chamber should become too much cooled down the draught should be let on, and any deficiency of dry fuel should be supplied to the fire chamber. Under proper management little or no dry fuel need be fed to the fire chamber after the operation is fairly commenced, the charred matter falling through the open grate will supply its place; and the caloric thus produced by the combustion of wet

fuel, will be vastly greater than from the same quantity by measure of the same fuel when dry. In the fire chamber and in the mixing chamber under intense heat the carbonaceous gases will decompose the steam from the wet fuel and effect complete combustion.

When the operation is fairly commenced if the water in the wet charge amounts to say fifty per cent by weight of the fuel, the dampers of the fire chamber should be nearly or quite closed to exclude the air; vapor from the wet charge will then descend through the corrugations and otherwise into the fire chambers and support the combustion therein, while other portions of the vapor will enter the mixing chamber and complete the combustion there. If the fuel, however, contains much smaller quantities of water, more air in proportion should be admitted at the damper, the object being to admit no more air than will supply the deficiency of the vapor.

In the drawings, *D* represents the chambers for the dry fuel, *W* those for the wet, *M* the mixing chamber, the dotted line *m* in Fig. 3 limits it for the wettest bagasse, P the partition, F the feed openings for the wet fuel with their doors, B the boiler, *b* the bridge. Little if any of the boiler should extend over the mixing chamber. If any considerable portion of the mixing chamber is covered by the boiler its cooling influence will prevent the decomposition of the the vapor and defeat the object of my invention. Great care should be observed in giving proper dimensions to the mixing chamber, for the perfection of the combustion and the efficiency of the furnace depend greatly upon it. The principal object of this chamber is to give the combustible carbonaceous gases from the fire, and the aqueous gases from the mass of wet fuel an opportunity of mingling together in such a manner and under such circumstances that the aqueous vapor will be decomposed by the carbonaceous gases, and its oxygen given out to complete the combustion of the carbon, without the introduction of air into the mixing chamber, thus saving the caloric previously communicated to the wet charge, while drying it and charring its lower portions, and avoiding the cooling influences of cold air. This can take place effectually only in the presence of a high degree of heat and in the absence of a supply of free oxygen. If this chamber be too small to receive these gases as fast as the furnace is able to produce them the operation will of course be choked and impeded. If the chamber is larger than can be kept densely filled with these gases, of course atmospheric air will be found there at the commencement, and will continue to find its way into the chamber, and while atmospheric air is present, the carbonaceous gases will take their oxygen from that source principally instead of decomposing the steam, and the heat in the chamber will be much diminished and the large quantity of nitrogen $\frac{4}{5}$ contained in the air, which is neither a combustible nor a supporter of combustion, will at once greatly increase the volume of gases to be sent forward to the stack and proportionably decrease its temperature; and when the chamber becomes very large the cooling influences become so great that combustion will immediately cease, and smoke mingled with steam oxygen and nitrogen, will go forward, thus wasting the fuel and imparting only a faint degree of heat to the boiler.

I have therefore fixed the size of the mixing chamber by many careful experiments—and that given above will produce the desired effect with

wet bagasse. For dryer fuels furnishing less vapor, the mixing chamber should be proportionably increased in size to supply the defficiency with air and to effect complete combustion. Rules more precise would be inconsistent with the nature of the subject.

A large and hot fire should always be preserved in the fire chamber below the grate, and directly under the charge of wet fuel, for the purpose of driving the vapor out of it and charring its lower portion—and the grate is left much more open than in furnaces for burning dry fuel of the same size, for the purpose of allowing the charred portions of the wet charge to fall through to supply fuel for this fire as fast as it becomes fit for that purpose, thus consuming the mass with little or no expenditure of other fuel.

What I claim as my improvement in furnaces for burning bagasse and and other fuels too wet to be conveniently burned in the usual way and well known ways is:

First, the combination of two chambers, the one above the other, and separated by a grate, the lower one for the combustion of any known dry carbonaceous fuel, and the upper one in immediate proximity therewith to receive heat therefrom for heating and drying the charge of wet fuel, with a mixing chamber, into which both continuously and simultaneously discharge their gases before reaching the thing to be heated, for mingling and mutual combustion.

I also claim in combination with said fire chamber and wet fuel chamber or drying chamber making the grate upon which the wet charge rests sufficiently open to allow the lower portion of the wet charge as it becomes dried and charred to fall through into the fire chamber and keep a hot fire therein, supplying the place of other dry fuel, while the uncharred portions of the wet fuel is properly supported by the grate till dried as described.

I also claim placing the mixing chamber of combustion in substantially the same position described relatively to the fire, and the wet charge, so that the products of combustion from the dry fuel may pass along the lower part of the wet charge, drying and charring it on their way to the mixing chamber, and reach it without being in any considerable degree obstructed or cooled by the wet charge substantially as shown.

I wish it distinctly understood that I make no claim to any of the parts or combination above specified except in their application to the preparation and combustion of wet fuels."

It will be observed that in this mode of combustion the wet fuel is subject to a constant process of distillation by the fire in the ash pit. The products of this distillation react on each other in the mixing chamber in the manner already described, while at the same time a portion of watery vapor is decomposed in the ash pit.

Theoretically no more heat can be generated in this mode of combustion than is consumed in the transformation of water into steam and the conversion of fixed into volatile products. But it is by no means a matter of indifference whether the oxygen requisite for complete combustion is drawn from the atmosphere or is derived from the decomposition of water by carbon

and its oxyd. In the former case, not only is there a great loss of heat carried away by the inefficient nitrogen of the air, but the diluted oxygen can never produce so intense a heat with the carbon as is the result of the reaction of the nascent oxygen with that element. Although Mr. Thompson was no chemist, he did not fail with his natural acumen to perceive this advantage and in his earliest patent he remarks: "After ample experiments I have discovered that any results that can be produced, by the use of dry fuel are inferior (to those obtained from my process) in proportion to the quantity used, and that results like mine can only be obtained by the use of wet fuel * * * fed into an intensely heated chamber: under such circumstances the water in the fuel in presence of the carbonaceous substances in the furnace will be decomposed, giving its oxygen to the carbonaceous matter, dispensing with a draft and its cooling and wastful influence and rendering the combustion so perfect that no smoke is visible."

Although this mode of combustion of wet fuel is now in use on many sugar plantations in Louisiana, and in some Tanneries of Pennsylvania and New York, no notice of it has so far as I am aware appeared in the scientific Journals. I am not without personal experience of its operation on a large scale, having in 1857 enjoyed the opportunity of studying carefully the management of one of Thompson's furnaces in three compartments (similar to Figs. 1 and 2) built for the combustion of wet peat. That fuel contained over seventy-five per cent of its whole weight of water and was too wet for the best results. But with the use of one-fourth part of dry wood, even this extremely wet and otherwise valueless fuel was rendered efficient, three cords (of 128 cubic feet) of wet peat and one cord of dry wood doing the work of four cords of dry wood in driving a steam boiler.

ART. XXVI.—*Note on a case of Artificial Crystallization of Metallic Copper and Dinoxyd of Copper;* by J. W. MALLET.

A FLASK, in which nitric oxyd had been prepared from nitric acid and scraps of copper, was allowed to remain over night by the pneumatic trough—the end of the gas-delivering tube dipping under the surface of the water. On the next day several scraps of copper were observed dotted over with very minute but brilliant crystals of metallic copper, which under the microscope proved to be octahedrons and combinations of the octahedron with the cube and dodecahedron, of various sizes—the largest measuring about twelve-hundredths of a millimeter along an octahedral edge. Along with these crystals of copper there were little cubes of the dinoxyd of copper in great abundance—

the latter not more than two or three hundredths of a millimeter on the edge, translucent, and of a splendid garnet red color.

On examination it became evident that these two bodies had been deposited in the crystalline state in consequence of the formation of the so-called "Bucholzian circuit"—one solid and two liquids so arranged as to produce electric action. As the flask cooled down the water from the peneumatic trough gradually rose in the tube, and in time ran down into the flask. The latter being in an inclined position the water flowed gently down the neck and formed a distinct stratum above the strong solution of nitrate of copper. Some of the scraps of metal which had projected above the surface of the solution, were now immersed, partly in the latter and partly in water, and it was upon the lower portion of each of these scraps—the part immersed in solution of nitrate of copper—that crystallization had taken place.

The deposition of metallic copper under similar conditions was observed by Bucholz, and other metals may, as is well known, be thus crystallized from solutions of their salts. The crystallization of the dinoxyd of copper was effected by Becquerel, but the arrangement of substances in his experiment was different from that now noticed—he placed a strip of copper in a saturated solution of the nitrate, the lower end of the strip touching some protoxyd of copper at the bottom of the solution. The crystals which he obtained were octahedral, not cubic.

Art. XXVII.—*Review of Dr. Antisell's Work on Photogenic Oils, &c.*

[Concluded from page 121.]

In describing the methods of purification proposed by Selligue, we shall make no attempt to follow their various details, our limited space compelling us to content ourselves with only the broadest generalities. Selligue sets forth at length two methods: 1st. A cold treatment which consists in agitating the oil with sulphuric, muriatic, or nitric acid. This agitation should be thorough, he says, and should be continued for a longer or shorter time according to the nature and quantity of the matter treated. Here follows a description of his agitators. After several hours' repose, the oil may be decanted, except from muriatic acid in which case more time and a larger amount of acid is required. After the oil has been thus separated from the deposit of tar, the acid remaining in it must be neutralized by means of an alkali. I prefer, says Selligue, to employ the lye of soap-boilers marking 36° to 38° [B.?], since it is easy of application, and produces a sure effect; I thus precipitate together the coloring matter and tar which would otherwise have remained in the oil. The oil is then decanted: if it is the first distillation of the crude oil I do not allow the mixture to subside entirely, preferring to leave a portion of the alkali mixed with the oil, and to distil off only $\frac{4}{5}$ths of the latter. * * * When the soda lye—in quantity slightly greater than is necessary to neutralize the acid—is added, the liquid must be agitated violently in order that each particle of the oil may be brought in contact with the alkali; this agitation must be continued until the color of the oil undergoes change.

The oil becomes less odorous and less highly colored after each such "cold treatment."

After having been allowed to separate from the lye, the oil is decanted off; if it has not lost much of its color the process has been badly conducted. It should be stated that the oil must not be agitated several times with the alkali, for, by so doing, the dark color of the oil would be restored. * * * As for the residues of the soda treatment, continues Selligue, they should be allowed to stand at rest during some days beneath a portion of oil, which will protect them from contact with the air; the clear lye at the bottom being then drawn off may be used for other operations, while the remainder is a soap, containing excess of alkali. By adding to it a little grease a soap can be made, or by adding water, grease may be separated. This grease is similar to that used for wagons.

2d. A warm treatment which follows the cold, and consists of a series of fractional distillations,—special operations for the purification of the "light-stuffs," being resorted to. For the details of these we must refer to the original specification of Selligue—a truly classical document which should be read by every one interested in the manufacture of coal-oils.* Nor will our limited space permit us to cite the detailed "example" of his treatment which Selligue has described. We trust that we have already written enough to enable the reader to judge whether or no Selligue understood his business.

As for paraffine, Selligue obtained it by subjecting the oil to a low temperature in order that this substance might crystallize. The mixed oil and paraffine was then thrown upon fine metallic filters through which the oil flowed while the paraffine was separated. Or one may separate, he says, the oil by imbibition, but this occasions a great loss of oil and also requires more labor. * * With this specification the scientific discussion of the subject by Selligue appears to have ceased, yet in the same year he replies† to a note published by Chenot‡ who asserted that the oil of shale often contains arsenic, denying that arsenic can be found in the products from his own establishments. He again describes the locality and geological position of his shale, the method of distillation employed,—how the temperature is gradually elevated, &c.

This is of interest as showing that the manufacture of coal-oil in France was no ephemeral fancy, but for many years was a well established branch of industry. In this connection the scientific research, upon the commercial products of the distillation of bituminous shale, of Saint-Evre§ should also be mentioned. Contemporaneous with Selligue we find other inventors occupied with the same subject. Thus Holthorp,‖ in 1841, claims that he has first discovered a means of purifying the fluid substance, which he calls "schiste," resulting from the distillation of coal or of bitumen. His attention was evidently chiefly devoted to the volatile naphthas, but he also obtained paraffine.

Guillard Meynier,¶ in 1842, speaks of the fixed oil from shale, telling us that it may be used for lighting or lubricating and that paraffine may be separated when the oil is cooled or treated with alcohol.

In the same year Bonnet** in treating of liquids suitable for lighting incidentally mentions eupion and paraffine.

Nor should we omit to mention the very interesting article upon *Hydrocarbures Liquides*, by A. Mallet (in Laboulaye's Dictionnaire des Arts et Manufactures, 2d Ed., Paris, 1854††), in which Selligue's processes are incidentally

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* A tolerably accurate English translation of this important patent may be found in the specification of M. A. B. B. Du Buisson, 1845; specification No. 10,726 of the English Patent Office.

† *Comptes Rendus*, 1845, xx, 573.

‡ *Ibid*, xx, 306.

§ *Comptes Rendus*, 1849, xxix, 339.

‖ *Brevets d'Invention*, liii, 263.

¶ *Brevets d'Invention*, lxxviii, 91.

** *Ibid*, lxxix, 63.

†† A portion of this article, which directly refers for the most part only to the volatile products suitable for "burning fluids," which may be obtained in any way from coal, is also contained in Dingler's *Polytechnisches Journal*, 1847, cvi, 128.

described. After discussing in detail the light volatile products obtained by distilling coal-tar, he says, we have still to speak of the carbo-hydrogens from shales; a branch of industry which we owe entirely to Selligue—cut off, alas! prematurely, in the midst of his career so full of discoveries and of useful works. As is well known, he obtained by distilling shales from the environs of Autun: I, volatile ethereal oils, II, fixed oils, III, oils combined with paraffine from which he prepared grease for carriages, IV, paraffine suitable for making candles, &c. Among all these bodies, Mallet continues, we have only to occupy ourselves with the volatile oils. Further on M. remarks that the acid and alkaline treatment used by Selligue is similar to that proposed by Barral for products from coal-tar. Thus far, says Mallet, these hydro-carbons have found no application,* partly on account of their insupportable odor when not purified and partly on account of their high price—about $10·00 the hundred lbs.—when purified.

We have been at no pains to ascertain whether the industrial distillation of shales, so well grounded by Selligue, has been continued in France without interruption up to the present time, for we know of no reason to doubt the fact. Certain it is that coal-oils produced by French manufactories were exhibited, at the Exposition Universelle at Paris in 1855, and likewise in 1851 at London.†

To any one familiar with the extreme slowness with which the practical applications of chemistry are even now imparted to, and recorded by, scientific writers, it would have been no matter of surprise if the results obtained by Selligue had remained uncopied upon the records of the French patent office. Such however was not the case. From the preceding citations it will be seen that his results were published in various well known journals and were widely diffused. Dumas, in his *Traité de Chimie Appliquée aux Arts*,‡ expressly calls attention to them. They are also noticed in the *Handwörterbuch der reinen und angewandten Chemie, von* Liebig, Poggendorff u. Wœhler, 1844, iii. 364. What we cannot explain is the apparent ignorance of these facts which was exhibited by several of the leading chemists of Great Britain on the occasion, of a trial,§ Young, *v.* White and others held in June, 1854, in the Court of Queen's Bench before Lord Chief Justice Campbell.

Several patents for the production of oils [coal-oils] from bituminous substances were meanwhile obtained in England. Butler,‖ for example, in describing his "improvements in the manufacture of oil and gas" proposes to distil bituminous shales, &c. for the purpose of obtaining oil and gas free from naphthaline. The shale, best after wetting it with water if the principal object is to obtain oil, is distilled in common gas retorts under which a gentle fire is lighted. As soon as oil begins to flow over freely the fire is to be increased and the retorts brought to a red heat; a large quantity of gas is thus obtained which is collected in a gas holder. The rough oils, as Butler informs us, may be purified by washing with sulphuric acid, filtration, &c., or they may be used in the rough state for making oil-gas. The oils in their rough state are often found entirely free from oxygen, and if obtained by the process described never contain so much as is contained in the coal-tar obtained in the coal-gas works where the coal is thrown into retorts already brought to a red heat. These oils in their rough state are further distinguished from coal-tar by their containing no naphthaline. Moreover the less volatile part of the

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* It will be observed by the reader of Mallet's treatise that he is interested only in a single branch of the subject, viz., the volatile naphthas—"light stuffs," just as we are here giving prominence to another portion of it, viz., the fixed, or paraffine-oil; and that he holds the naphthas from shale in small repute, since in his opinion they can never compete in the matter of cost with those from coal-tar.

† A. U. Moreau (No. 1361, Cat. 9), *Bas-Rhin.*

‡ Paris, 1844, t. vii, p. 390; also t. iii, p. 315 of the Liege edition; and B. vii, S. 510 of the German translation.

§ Reported in Barlow's London Journ. of Gas Lighting, Aug. 10, 1854, vol. iii, p. 508.

‖ Patent granted Jan. 29, 1833. Specification No. 6375 of the English Patent Office.

oil [No. 2] offers another characteristic feature; if after being drawn off and distilled, and if in this latter process the more volatile or first proceeds, say one half of the quantity acted upon, be set apart and the remaining half exposed to a low temperature, there will soon appear in this part of the distilled oil small flakes of a white, odorless, and light substance which is a compound of carbon and hydrogen [paraffine]. The familiarity with the subject, somewhat remarkable in view of the early date of his patent, which Butler exhibits cannot fail to strike the reader. This inventor was however unfortunate in the idea of trying to make at the same time oil and gas—in endeavoring to reconcile two antagonistic processes.

In 1841, Sept. 4, Count de Hompesch of Prussia* specified certain "improvements in obtaining oils and other products from bituminous matters."† It is well known, he says, that oils may be obtained from these substances but from the imperfection of the processes now used the quantity obtained is small, the quality inferior, and the smell noxious. My invention consists in an improved process, whereby I increase the quantity, improve the quality, and remove or greatly modify the smell. I have found by experiment, he continues, that the oil from shale, &c., possesses three different characters which may be called essential oil, intermediary fat oil, and thick oil, and these oils I separate by means of peculiar apparatus—which he describes in detail. In distilling shales heat is applied until the temperature reaches 100° R. = 257° F., at which temperature essential oil will pass over. The charge, after having been subjected to this temperature for half an hour, is pushed forward in the retort which is now subjected to a heat of 200° R. = 482° F., by which increased heat the intermediary or fat oil is obtained. After having subjected the charge to this increased temperature for half an hour the workmen again pushes the charge further on in the retort where it becomes of a red heat; the vapor now given off yields the thick oil. The carbonization is now complete; and I obtain these three separate oils by the gradual increase of the heat; and I effect this distillation without decomposition of the substance, the vapors escaping from the retort as fast as they are formed.

The essential oil is separated from the fat oil by exposing the mixture to a current of steam by which the more volatile oil is carried off. The oil [fixed] thus prepared must be filtered and is then ready for application to all kinds of machinery, being very fat, works without friction and leaves no sediment. The essential oil is collected and subjected to further treatment.

The specification of Du Buisson‡ for improvement in the distillation of bituminous substances, is an almost literal translation of Selligue's last patent. Indeed, Du Buisson tells us that the extensive works at Autun, Department of the Saone and Loire, France, are partly his property and that he has the management of them as chemist. He affirms moreover that the most important results have there been attained — results which place the distillation and treatment of schistus among the most useful and productive of chemical manufactures.

Since we have already extracted largely from Selligue's specification it is unnecessary to cite more of it here. It is a little curious that this most important patent is not mentioned in Dr. Antisell's "list of English Patents" (p. 141).

The well known attempts to prepare paraffine and oils from peat§ need not be discussed here. Another patent, not mentioned in Dr. A.'s list, is that of

* Specification No. 9060 of the English Patent Office.

† In a "memorandum of alteration," dated July 5, 1842, de Hompesch claims the right of distilling "bituminous schists, shales, or slates, or other rocks or minerals containing bitumen or bituminous substances."

‡ Dated June 23, 1845. Specification No. 10,726 of the English Patent Office.

§ Antisell, p. 85; compare Rees Reece's patent dated Jan. 23, 1849. Specification 12,436 of the English Patent Office.

George Michiels.* It is peculiarly interesting since a portion of it relates to the preparation of oils from caking coals. Michiels proposes in fact to prepare coke from bituminous coals, and from mixtures of such coals with anthracite, by moistening the powdered coal with water and introducing it—in charges of six tons—into brick retorts furnished with ordinary condensing apparatus and other appliances. The retorts are then heated as if it were intended to produce gas, with this difference, that the temperature for the first fifty hours should not exceed nascent red heat, or 964° F.; after that time it should be increased progressively until it attains a clear red heat, which would be about the ninety-sixth hour, I should remark, continues Michiels, that about the sixtieth hour I shut off the communication between the retort and the condensor by closing the hydraulic valves, and at the same time open the valve on top of the retort, &c., so as to allow the air to enter, which burns the hydrocarburets [now being evolved] and the products of that combustion heat the retort, &c. in passing through the flues which surround the retort. * * * I thus obtain coke, ammoniacal liquors and liquid hydro-carburets. These "hydro-carburets" were repeatedly distilled by M. in order to obtain as much light volatile oil as possible, A heavy yellow oil of density 0·911, or lower, was also obtained which according to M. will be found applicable to many useful purposes, and is suitable for his principle object of turning into gas.

Further on (p. 15 of his specification) Michiels explains that this oil is well adapted for manufacturing gas upon a small scale, since the gas prepared from it requires no purification, and since it can be used in any of the ordinary apparatus for making gas from camphene, oil, or resin. In a word, he proposed using it just as rosin oil is now so largely employed by private gas-works in this country, or as Boscary and Butler had used the same coal-oil before him.

We pass† to a consideration of the well known labors of Mr. James Young of Glasgow.‡ From evidence brought forward in the trial already cited it appears that Mr. Young's attention was called in 1847 to a mineral oil [petroleum] found exuding from a coal pit at Riddings in Derbyshire. From it he obtained a good lubricating oil which he continued to prepare as long as his supply of petroleum lasted. Occupied as he was with the subject it can surprise no one that he should soon have turned his attention to the distillation of the highly bituminous mineral of Torbane-hill, now known as Boghead coal in England and in this country, which was introduced to public notice in 1850.§ From this substance Young was enabled to prepare a much larger amount of oil per ton of mineral than had been obtained by any of his predecessors. To the discovery of the vast source of an admirable raw material which the Boghead mine furnished is evidently due the immense increase in the production, and of course consumption, of coal-oil which immediately ensued. To this we say, more than to anything else is to be attributed the rise and progress, during the past few years, of the almost innumerable manufactories of coal-oil on the continent of Europe and in our own country. From the impetus thus given, a branch of industry which had long been, comparatively speaking, of only local importance soon attained an enormous development.‖

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* Granted April 30, 1850. Specification No. 13,066 of the English Patent Office.

† Making no pretence, be it understood, that we have been able to collect all that has been published upon the subject before 1850.

‡ Patent dated Oct. 7, 1850.

§ According to Mr. T. G. Barlow, *London Journal of Gas Lighting*, iii, 519.

‖ We cannot, in this connection, forbear quoting the following pertinent remarks from Lord Campbell's charge to the jury in the case—Young *v.* White and others (see *London Journal of Gas Lighting*, iii, 521)—already cited.

"And this brings me to an observation," says his Lordship, "which I meant to make, and which I should have been sorry if I had forgotton, which is this—that it was the discovery of this Boghead coal that seems to have given the great value

Let it be distinctly understood that we would in no wise detract from the real merit of Mr. Young. Uniting, as he does, no small share of chemical knowledge with the cautious, untiring energy of his countrymen, few men could have been found better qualified to grasp the golden opportunity of which he so fortunately availed himself. His name must ever remain associated with those of the distinguished observers from whose labors this most important branch of industry has resulted. It is Dr. Antisell only whom we blame for his incorrect and partial "history." When, for example (on p. 14), Dr. A. tells us that: "only since the year 1850 has the manufacture of paraffine from pit-coal, turf and bituminous shales succeeded as an art. The first manufacture was that of James Young in Manchester, by whose process, from 100 parts of Cannel-coal 40 per cent of oil and 10 per cent of paraffine could be obtained." He makes a statement which is grossly exaggerated—if not *entirely* at variance with fact—as our readers must already have perceived.

We willingly quote what follows: "In thus showing [*i. e.*, dogmatically *asserting*] that the practical manufacture of oils from coal is due to James Young, it may not be amiss to call attention to what it was which he produced from coals by distillation. He claimed the production of paraffine oils—not the production of naphtha or benzule [benzol?], nor naphthalin, but paraffine and its congeners: this involves the slower distillation of coals at a lower temperature than had been hitherto effected, and this novelty in practice was followed by the novel result of a copious production of isomeric liquid hydrocarbons; so that really two great results were first demonstrated, practically by the operation of Young's process, namely—1st, That coal was a material from which liquids could be manufactured economically, as tar, bitumens, and schists had been hitherto employed; and 2nd, That the liquids so formed were paraffine-containing compounds." Having merely to suggest that the sentence might have been more tersely put. For in truth it means only—if it means anything—that in the opinion of Dr. A., Mr. Young was the first person who distilled coal [on a manufacturing scale?] at comparatively low temperatures. What Dr. Antisell's private views regarding "low temperature" or "practical" may be, we are ignorant. But we do know that when, 30 years ago, Reichenbach distilled quantities of coal of 75 lbs. weight each, and exercised the greatest care in maintaining the temperature of his retort at as low a degree as was admissible, as he has most minutely described in the memoirs which we have already cited;—when he obtained paraffine and eupion as results of his operation; he most certainly demonstrated the practicability and the manner of preparing both paraffine and "paraffine-oil."

All this however does not appear to satisfy Dr. A. in the least degree, who repeatedly assures his readers that the manufacture of oil from *coal* dates from the patent of Mr. Young. Since our author has seen fit to dwell at length upon this point and to devote so much space to its discussion we may be pardoned for referring to it here.

As is well known the term "coal" is applied in common language to a great variety of mineral combustibles no two kinds of which are precisely alike while some sorts are exceedingly unlike others. The term is at best merely conventional; used, in lack of any better one, to designate substances with the real nature of which we are still almost entirely ignorant.

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to paraffine, because until then I do not find it was obtained in such quantities as really were of any considerable value; but the Boghead coal now being discovered, and this schist or coal being discovered, which is of very rich quality, and having a great deal of what is the essential part of the paraffine; from that time it has become much more important; and that may explain why, although the mode of obtaining paraffine was before well known, it should not have been put in practice because it would not appear that it could be put into practice with much profit or benefit, unless you had such a substance as Boghead coal on which you could operate."

In confirmation of this view compare also: Payen, *Précis de Chimie Industrielle*, 4e Ed.. Paris, Hâtchette. 1859. t. ii, p. 685.

With the flint-like anthracite of Wales, the beautiful Albert coal*—but a step removed from asphaltum—of Hillsboro, New Brunswick; our common lignites, frequently shading into peat, and the bituminous shales as frequently passing by insensible gradations into common slate, as points upon its extreme limit, we have within the circumference of the circle an infinite number of substances, shading into each other by scarcely perceptible degrees,—all of which are, in technological language at least, varieties of *coal*. The "amplitude of variation" which this species, or rather this term, enjoys is indeed so great that it would be a matter of no small difficulty to choose any single member of the medley as a central point, or even to conceive of an ideal coal to which all other varieties should be referred. In attempting any such selection a native of one of our sea-board states would assuredly lean towards anthracite; the South-German towards his excellent lignites; the Scotchman towards his cannel; while Newcastle would claim a proverbial right of precedent. We would, for our own part, vote for the last named, or some other good caking coal, capable of furnishing both gas and serviceable coke, and of being used for an infinite variety of purposes. Starting from this then as a type, observe, that as we pass towards the cannels, the different varieties of coal become better and better suited for the manufacture of gas or oil, *i. e.*, they contain more and more hydrogenous compounds. The appearance of the mineral meanwhile approaching more and more closely to that of slate, while at the same time the value of the fixed carbonaceous residue becomes less and less, soon ceasing to be "coke" at all, but rather a more or less carbonaceous slate. At length a maximum of hydrogenous matter is reached as in the case of Boghead coal, a slaty substance, the fixed residue from the distillation of which is a slightly carbonaceous stone, valueless as fuel and useful only, as a substitute for bone-black, for purposes of disinfection or decolorizing liquids, uses to which the residues of the French shales have long been applied. Beyond this maximum, as the amount of gas and oil-producing substances diminishes, and the amount of earthy matter increases,—taking the place of the fixed carbon in our typical caking coal, we pass into "bituminous shales," and these become less and less bituminous until at length we reach common clay slate containing no organic matter whatsoever. We have here traced no fancy sketch. That the "cannel coals" thus gradually pass into "bituminous shales" is now well enough known, at least to gas engineers and other practical observers. It would not for that matter be exceedingly difficult to obtain a continuous series of specimens exhibiting this almost insensible gradation. Now did Mr. Young devote his attention to the distillation of caking coals similar to our typical Newcastle? By no means! On the contrary we find him occupied with a mineral which was called indifferently "shale" or "coal," until it was in 1853 decided in a Scottish court that it should henceforth be legally known as coal.†

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* On page 18 of Dr. A.'s work the following remark occurs. "In one respect they [bitumens] differ from coal. In no case can an organic tissue or structure be demonstrated when they are examined under the microscope. Viewed in this light the mineral found at the Albert mine, New Brunswick, should be classed as a bitumen since Dr. J. Leidy was unable to detect any trace of structure in its mass." We cannot understand the motive of our author in thus again dragging to light this negative result, for it is perfectly well known to the scientific men of America that Prof. J. Bacon detected the existence of vegetable structure in the interior of masses of the Hillsboro coal. See *Reports on the Geological Relations, chemical analyses, and microscopic examination of the coal of the Albert Coal Mining Co., situated in Hillsboro, Albert Co., N. B.*, by Chas. T. Jackson, M.D. New York: printed by Nesbitt, 1851, p. 30; compare this Journal, [2], xiii, 276.

† We had supposed, when the above was written, that the decision of this court had been sustained. That in this we were mistaken appears from the following, which we extract from a statement in a late number of the *London Journal of Gas*

We would cast no reflection upon the judgment rendered in this famous suit. Looking at it as a mere matter of equity, depending upon the business relations of the parties at issue, this verdict was in our own opinion, just. But the fact of this mineral being, or not being, called a *coal*, does not in the least degree prevent it from being *also a shale*; and that it is more nearly related to the shales than to the coals is believed by a large proportion of those who are intimately acquainted with it, be they scientific or practical men. We have no space to discuss at greater length this quibble of Dr. Antisell's—which, however excusable it might have been in a retained attorney or solicitor of patents, is anything but becoming to the chemical professor or the historian—being content to refer the reader to the published reports of the trial just mentioned. We will here cite only a few lines* descriptive of Selligue's mineral: "The quantity of oily matter in these shales is very variable and often very

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Lighting (Jan. 17th, 1860. vol. ix, p. 41), received as this article is going through the press.

"SETTLEMENT OF THE GREAT TORBANE-HILL CASE.

We have been favored with the following particulars connected with the well known case, the 'Bathgate or Boghead Gas Coal *alias* the Torbane-hill Mineral,' which has lasted upwards of seven years, having passed through several phases in the Supreme Law Courts of Scotland and England. A compromise was finally come to on Wednesday last, the eleventh current. It is embodied in a minute of agreement between Mr. and Mrs. Gillepsie of Torbane-hill, of the first part, and Messrs. James Russel and Son, and James Russel, Esq., of Blackbraes, of the second part.

The preamble of the minute of agreement, which itself consists of twelve articles, is as follows:

'The said parties, considering that disputes and differences have arisen between them and a lengthened litigation has taken place, with respect to the missives of agreement for a lease of certain minerals in the lands of Torbane-hill, entered into betwixt the said first party hereto on the one part, and the said company of James Russel and Son, and individual partners thereof, on the other part, and dated the 30th of March and 1st of April, 1850; and both parties being now desirous that the said litigation should be brought to an end, and all disputes and differences between them amicably adjusted and settled, they have agreed, and hereby mutually agree and bind themselves as follows:'—

The first two articles provide that the actions at present depending shall be abandoned, as a consequence of the execution of the minute.

The third article provides that each party pay their own expenses.

The fourth article, which has for title 'Name of Mineral,' is both an important and curious one—important in a scientific point of view, and curious as illustrative of the pertinacity with which either party have clung to their own views. The article is as follows:—'Whereas the second party have been, and are, working in the said lands of Torbane-hill, and disposing of, under the name of Bathgate or Boghead gas, parrot, or cannel coal, a mineral which they, the second party, deemed and deem to be a parrot or cannel coal, and which the first party deemed and deem to be a new mineral substance, having an argillaceous base, and to be of so peculiar a nature as to constitute it in truth a new and very peculiar variety of bituminous schist, slate, or clay, and have been for some time in use to call 'the Torbane-hill Mineral'; it is hereby agreed that, throughout the remainder of these presents, where the mineral in question is named, it shall be called for the sake of brevity the disputed mineral.'

Article fifth relates to the subject of a portion of Torbane-hill which was reserved from the operation of the mineral lease, and by this article 'the disputed mineral,' as we now call it, contained in the reserved portion of the estate, may be worked or let by the proprietors, without the danger of any obstruction being offered by the second party in the agreement." * * *

* From Dufrénoy et Elie de Beaumont's Explication de la Carte Géologique de la France. Paris, Imp. Royale, 1841. t. i, p. 673.

considerable. According to M. Xardel some rare samples exist which afford even 45 @ 50 per cent; * * * other specimens afford 20 @ 25 per cent. The beds which are worked, or are capable of being worked, yield from 5 to 9 per cent." [p. 675.]—Again [p. 676], "The impressions of fossils, so common in the shales of d'Igornay, occur in the poorer shales. The rich shales, on the contrary, often contain vegetable remains analagous to those commonly found in the coal measures. Perhaps the beds of rich shale are in a manner the representatives of coal-beds; it is to be remarked that in the shale which yields 9 per cent of oil its sheets are covered with a multitude of shining (*miroirtantes*), lenticular veins, having a waxy fracture, which by their aspect and manner of burning recall the variety of coal which is called *cannel coal*." —Leaving it for our readers to answer the question; how far removed in anything but productiveness is the "coal" (Boghead) upon which Mr. Young has operated from the "*shale*" distilled by M. Selligue?

It may not be amiss to mention the fact that upon the continent of Europe the Boghead mineral is almost universally called, not coal, but shale. A fact with which the reader can readily enough familiarize himself by consulting the German chemical journals of the last eight or ten years. In proof of it we cite only the following:* "The recent verdict in the celebrated Torbane-hill-mineral case appears to be contrary to the scientific opinions held in Germany, as proof of which we have a case in point, and which, although not at the time known in this country, was officially decided upon in Berlin previous to the trial coming on in Scotland, which terminated on the 4th of August last. It appears that in Frankfort-on-the-Main there has, for some time past, been in existence a company for lighting the streets and houses by gas from oil, resin, &c. A rival English company contracted to light with coal-gas; and to give both fair play, it was decided that the latter company should be confined to the use of coal alone. Mr. Engelhard, the manager of the Oil (Resin) Gas Company, having heard of the Boghead and Torbane mineral, obtained specimens, and having found they produced excellent gas, gave an order for a large consignment which reached Frankfort *viâ* Rotterdam, through a Dutch agent. This was entered at the Custom-house as cannel coal, much to the annoyance of Mr. Engelhard, who was no more at liberty to make gas from coal than his rivals were to make it of anything else but coal. He was, however, prevented from the necessity of a trial at law, for the officials did not feel themselves justified in charging the duty as coal, although, as other mineral, it would pass free, and applied to higher authorities for instruction. These parties were as much at a loss as their inferiors, and the case was eventually transferred to the Central Board of Customs at Berlin, the last court of appeal of the Prussian *Zollverein*, where all disputed questions in the German States are settled. Scientific men, connected with the board, examined the Boghead and Torbane mineral and decided that it was *not* coal, but *bituminous shale*, which is said to be the general opinion among German chemists. It has been admitted into Germany, duty free, and Frankfort is now partially lighted with gas from this mineral, charged for as resin or oil gas. It is described as a clay containing bitumen, and producing oil when heated. At all events, we may take this German decision as impartial as, had it been admitted as cannel coal it would have been subject to a duty varying from 1*s.* to 1*s.* 6*d.* per ton."

In connection with the question of the products of the distillation of *coal* which Dr. A. would have us believe so entirely new to the world and to this country in particular, we cite the following from The *Encyclopædia of Chemistry*, by James C. Booth and Campbell Morfit. 8vo, Philadelphia, Baird, 1850, p. 461. Article, Coal: paragraph, "products of dry distillation."

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* See *London Journal of Gas Lighting*, Oct., 1853, iii, 256; from *London Mining Journal*.

"These products [of the dry distillation of coal] are somewhat analogous to those derived from wood, and some are identical with them. * * * The liquid products consist of various bodies closely allied to petroleum, and the solids *Naphthaline* and *Paraffine*. The relative proportions of these products vary with the temperature. The lower the heat employed, the less gas, and the more solids and liquids are produced; the higher the temperature the greater is the quantity of carburetted hydrogen." Before closing this sketch we must refer to and correct a palpable error of Reichenbach's* which has been cited by Dr. Antisell (p. 14), from whom we quote it: "So remained paraffine until this hour [date of Y.'s patent], a beautiful item in the collection of chemical preparations; but it has never escaped from the rooms of the scientific man."

Upon the reader who has followed us thus far we need not urge that the above statement is incorrect. As an offset to it we cite the following:†

"In the Parisian Industrial Exhibition for 1839 Selligue exhibited: 1st—Bituminous shale, then fluid bitumen [crude oil], mineral grease, crude and refined (the former at 50 fr. the 100 kilog. [=$5.00 per 100 lbs.]), also *mineral wax* [paraffine] crude and refined (the former at 125 fr., the latter at 180 fr. [=respectively 12½ and 18 dollars per 100 lbs]). The purified mineral wax was beautifully white but the candles made of it had a soiled appearance." V. Hermann (now, according to Wagner, councillor of State in Munich goes) on to assert that "if these fatty products can be prepared economically they belong to the most important objects of the Exhibition."

It would be foreign to our purpose were we to attempt to trace the recent history of the art of manufacturing coal-oil, even if our space allowed of it. During the past few years a large number of papers‡ on the subject have been published in the Scientific Journals of Germany; while several recent works upon the materials used for producing light have each devoted a separate chapter to its description. A few special treatises have also been published of which the following is a, doubtless very incomplete, list.—§

Uhlenhuth, Ed. *Handbuch der Photogen- und Paraffin-Fabrikation aus Torf, Braunkohle und bituminösem Schiefer nach den neuesten Versuchen und Erfahrungen. Quedlinburg, Basse*, 1858.

Mueller, Carl, Georg, *Die trockene Destillation und die hauptsächlichsten auf ihr beruhenden Industriezweige.* Leipzig, Barth. 1858.

Danckwort, Meitzendorff *und* Wernecke. [Committee of the Magdeburg *Gewerbeverein.*] *Ueber das Photogen oder Mineralœl, so wie die ihm*

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* Erdmann's Journal für praktische Chemie, lxiii. 63. Did our space allow, we would gladly transcribe the whole of this article—an English translation of which may be found in the London, Edinburgh and Dublin Philosophical Magazine, [4.] viii, 463—in proof of our assertion that the present widely-spread manufacture of coal-oil and paraffine is mainly due to the comparatively recent discovery of rich stores of highly bituminous substances.

† From v. Hermann's *Die Industrie Ausstellung zu Paris im Jahre*, 1839, *Nürnberg*, 1840. p. 147;—in Wagner's *Jahresbericht ueber die Fortschritte der chemischen Technologie*, 1855, i. 416.

‡ Very complete synopses of these may be found in Wagner's *Jahresbericht*, four volumes of which have thus far been published. For references to the recent admirable *scientific* researches of Greville Williams, De la Rue, and others, which have been chiefly confined however to the more volatile portions of the oil and to the basic compounds which occur in it, see Liebig and Kopp's *Jahresbericht der Chemie, u. s. w.*

§ Small as this list is, it will nevertheless recall to the mind of the reader the modest lines with which Dr. Antisell's preface commences, namely these: "the present little treatise is the first published monograph on the art of distilling oils from minerals containing Bitumen."

ähnlichen Leuchtstoffe, in Bezug auf ihre Feuergefährlichkeit und ihre Anwendung. Magdeburg, 1856.

Also the insignificant brochure of SCHRADER, F. W. *Ueber die industrielle und national-ökonomische Bedeutsamkeit der Gewinnung von Chemikalien insbesondere des Paraffin's und Photogen's aus dem Kohlentheer, u. s. w.* Aschersleben, Beyer, 1856.

This article must here close. Leaving unnoticed several inaccuracies which we had intended to discuss we will dismiss the subject with two brief quotations. The first from Dr. Antisell's book, p. 15. "An impression has taken hold of the American manufacturing public that the patent of James Young has no force, as it was not a new invention at the date of the patent; and from the unfavorable effect of that patent upon the actual manufacture of coal-oils in this country, an ill-feeling has been produced against it. That the owners of this patent have not acted wisely by witholding sales and licenses under it until very lately, is to be regretted; but that it was a *bona fide* improvement in an art at the time when it was patented, and that therefore the patent was rightly issued in this country, there can be no shadow of a doubt in the mind of any one who carefully traces the steps of the discovery of the production of photogenic oils from different materials."

The second from Lord Chief Justice Campbell's charge* to the jury in the case already alluded to. "Now gentlemen I direct you, in point of law, that if there were books then [at date of Young's patent, 1850] in circulation in England disclosing this mode of obtaining paraffine and paraffine oil which were known, were accessible, that the patent would be invalid, although Mr. Young never read those books, and although that mode had not been actually put in practice. If there were books in England in circulation, accessible to all who were interested in the subject, which disclosed this, and would instruct them and enable them to obtain the paraffine and the paraffine oil from the distillation of bituminous substances, then Mr. Young's patent would be invalid."

FRANK H. STORER.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On a probable means of rendering visible the Circulation in the Eye;* by OGDEN N. ROOD, Prof. of Chemistry in Troy University.—Some time ago while looking at a bright sky through three plates of cobalt-glass, I saw with astonishment that the field of view was filled with, and traversed in all directions by small bodies resembling animalcules.

They were seen on the blue field as yellowish spots, and always appeared elongated in the direction of their motion, which was as a general thing tolerably uniform. The same result was obtained by experimenting upon the eyes of a number of persons.

Convex lenses of various foci, (from 3 in. to $\frac{1}{2}$ in.), were now held before the eyes, so as to give the blue light various degrees of convergence and divergence, without in the least altering the appearance of the moving bodies; this seemed to indicate that their locality was in the retina or in its immediate neighborhood.

A position near the axis of vision was selected, and observed, when it was found that these bodies in traversing this spot always pursued the same direction and path, disappearing at the same point; other positions near the axis gave like results.

* *Loc. cit.*, p. 520.

This would seem to preclude the possibility of the moving bodies being animalcules swimming in the humor of the eye; the most probable remaining supposition is, that they are blood-corpuscles circulating in the retina or in its immediate neighborhood. The apparent diameter of these bodies when seen projected on a window six feet distant may be about $\frac{1}{20}$ of an inch, which corresponds to about $\frac{1}{1600}$ of an inch on the retina. The average diameter of the blood globules is $\frac{1}{3200}$ of an inch, but taking into account the fact that the shadows of the moving bodies are not well defined the correspondence may be considered pretty satisfactory.

The question now arises as to the manner in which the blue glass renders the circulation visible, for these moving shadows cannot be seen with distinctness through red, orange, yellow, green, nor even purple media; they are on the other hand well shown by a certain thickness of a solution of the cupro-sulphate of ammonia.* Yellow solutions when combined with the blue glass or blue solutions render the circulation invisible, and it does not reappear till the yellow solution has been made so dilute as barely to preserve a yellow tint, and to transmit the spectrum almost unaltered. This shows that the indigo and violet rays are principally concerned in the production of this appearance, but that it cannot be attributed to fluorescent properties in the blood discs is indicated by the fact that the circulation can be seen through a considerable thickness of crown glass, through an infusion of red sanders wood mixed with ammonia, as well as through a solution of the disulphate of quinine.

The only explanation that has occurred to me as being probable is the following: the blood discs are yellow and consequently opaque to a great extent to the indigo and violet rays; they would therefore in passing before the retina cast shadows on it; now the retina being already strongly impressed with blue light, that portion of it which was momentarily protected from the action of this light, would experience the complimentary sensation—or would see instead of a moving shadow a yellowish moving streak. This explains also why the appearance is not seen with any distinctness in red, orange, yellow, or green light, for yellow media are to a great extent transparent to all their rays and therefore fail to cast shadows. These observations if new may be of some interest to those engaged in the study of the physiology of the eye.

Troy, May 14th, 1860.

TECHNICAL CHEMISTRY.

1. *Care of Platinum Crucibles.*—In connection with some sensible remarks upon the use of sand in cleaning platinum crucibles,—a practice which, with Berzelius, (Lehrbuch der Chemie, 1841, 4th Aufl. p. 516,) he heartily commends—urging that it should be employed every time that a crucible is used, ERDMANN explains the cause of the gray coating which forms upon platinum crucibles whenever they are ignited in the flame of Bunsen's gas-burner.

This coating has given rise to much annoyance and solicitude among chemists. Indeed it has often been asserted, that the use of Bunsen's

* Mr. Wm. B. Taylor of Washington to whom some of these facts were communicated by Prof. J. Henry was able to trace this appearance, though with *diminishing distinctness*, through plates of bluish-green and yellow glass.

burner is unadvisable in quantitative analysis, since by means of it the weight of platinum crucibles is altered and the crucibles themselves injured. The coating is produced most rapidly when the crucible is placed in the inner cone of the flame, and the more readily in proportion as the pressure under which the gas is burned, is higher. Having found it advantageous to maintain, by means of a special small gas holder, a pressure of four or five inches upon the gas used in his own laboratory, Erdmann has observed that the strong gas flame thus afforded, immediately occasions the formation of a dull ring upon the polished metal placed in the inner flame, this ring being especially conspicuous when the crucible becomes red hot; it increases continually so that after long continued ignition the whole of the bottom of the crucible will be found to be gray, and with its lustre dimmed.

This ring is caused neither by sulphur, as some have believed, nor by a coating of inorganic matter, but is simply a superficial loosening of the texture of the platinum in consequence of the strong heat; whence it first of all appears in the hottest part of the flame.

In conjunction with Pettenkofer, Erdmann instituted several experiments which have left but little doubt that the phenomenon depends upon a molecular alteration of the surface of the metal. If a weighed polished crucible be ignited for a long time over Bunsen's lamp, the position of the crucible being changed from time to time in order that the greatest possible portion of its surface shall be covered with the gray coating, and its weight be then determined anew, it will be found that this has not increased. The coating cannot be removed either by melting with bisulphate of potash or with carbonate of soda. It disappears, however, when the metal is polished with sand; the loss of weight which the crucible undergoes, being exceedingly insignificant, a crucible weighing 25 grams, having lost hardly half a milligram. When the gray coating of the crucible is examined under the microscope it may be clearly seen that the metal has acquired a rough, almost warty, surface, which disappears, when it is polished with sand. Platinum wires which are frequently ignited in the gas flame, for example the triangles which are used to support crucibles, become as is known, gray and brittle. Under the microscope they exhibit a multitude of fine longitudinal cracks which as the original superficial alteration penetrates deeper become more open, or as it were spongy—until finally the wire breaks.

If such wire is strongly and perseveringly rubbed with sand, the cracks disappear, and the wire becomes smooth and polished, for the grains of sand acting like burnishers restore the original tenacity of the metal; very little of its substance being rubbed off meanwhile. The loosening effect of a strong heat upon metals is beautifully exhibited when silver is ignited in the gas flame; a thick polished sheet of silver immediately becoming dull white when thus heated. Under the microscope, the metal appears swollen and warty. Where it has been exposed to the action of the inner flame along its circumference, this warty condition is visible to the naked eye. A stroke with the burnishing stone however, presses down the loosened particles, and reproduces the original polish. This peculiar condition which the surface of silver assumes when it is ignited, is well-known to silversmiths, it cannot be replaced by any etching with

acids; and it must be remembered that what is dull white in silver, appears gray in platinum.

If each commencement of this loosening is again destroyed, the crucibles will be preserved unaltered, otherwise they must gradually become brittle. Crucibles of the alloy of platinum and iridium are altered like those of platinum when they are ignited; it is, however, somewhat more difficult to reproduce the original polish of the metal, by means of sand, as might be expected from the greater hardness of the alloy.

The sand used should be well worn; when examined under the microscope, no grain of it should exhibit sharp edges or corners, all the angles should be obtuse.—*Journ. für praktische Chemie*, March, 1860, lxxix, 117.

2. *Safe and easy method of rectifying Sulphuric Acid;* by N. Neese of Kiev.—"The rectification of sulphuric acid offers an example of how readily even the most practical matters may fall into oblivion in spite of the copiousness and the vigilance of our literature. Fifteen years ago, Prof. Siller, at that time filling the chair of Pharmacy at Dorpat, called my attention to an exceedingly ingenious device for rectifying sulphuric acid, which he had read in some journal. The retort in which the rectification of the acid is to be conducted, should be placed upon a bed of ashes about an inch in thickness, and sand then heaped up all around it; the distillation may then be proceeded with, without any further precautions. By this method I have twice rectified sulphuric acid, operating with a very bad retort, upon portions of 15 lbs weight; and have been astonished at the facility and tranquillity with which the distillation proceeded. The ashes evidently act as a non-conductor, so that the acid can boil only from the sides of the retort. Since then, manifold expedients have been proposed, all depending upon some peculiar apparatus, constructed for this particular purpose, but I have not yet met with any reference to the simple contrivance which has here been mentioned."*—From *Archiv der Pharmacie*, cxlv, 267; in *polytechnisches Notizblatt*, 1860, xv, 43.

[Frederking of Riga, (*Archiv d. Pharm.*, Aug., 1859; in *American Journal of Pharmacy*, viii, 88,) corroborating the statement of Neese, states that he can recommend the process from his own experience,

* The author is evidently not aware that several devices almost as simple as the one which he describes have previously been proposed. Thus Otto, (in his *Lehrbuch der Inorganischen Chemie*, Braunschweig. 1855, i, 275,) directs that the retort be caused to rest upon an inverted crucible cover, which is placed upon a grate in an extemporaneous fireplace constructed of loose bricks, (figured in Otto,) so that the bottom of the retort may be less strongly heated than the upper portions by the charcoal fire which is built around it. Otto, however, considers it necessary to cover the retort with a thin coating of clay which has been mixed with a dilute solution of carbonate of soda or of borax, instead of water, so that it may adhere firmly to the retort when heated; urging that the danger of fracturing the latter is not only lessened thereby, but that a more rapid distillation is insured, since little or no condensation can occur within a retort thus protected. In Mohr, Redwood and Procter's *Practical Pharmacy*, Phila., 1849, p. 337, a somewhat similar proposal is made. A Hessian crucible being employed, however, instead of the cover used by Otto:—moreover, the retort is not coated.

Possibly a more convenient method than either of the above would be to fill a sufficiently capacious crucible with ashes, or some other non-conducting substance—calcined gypsum, for instance,—as a bed upon which to rest the bottom of the retort. A charcoal fire similar to Otto's being then built around it. F. H. S.

inasmuch as he has during the last twelve years, repeatedly rectified sulphuric acid in the manner indicated.—F. H. S.]

3. *Vulcanization of Caoutchouc, by means of mixed Sulphur and Hypochlorite of Lime.*—GAULTIER DE CLAUBRY, having detected the presence of chlorid of calcium in many samples of vulcanized rubber, and suspecting that this salt might have been derived from hypochlorite of lime employed in some modification of Parkes' process of cold sulphuring, [English Patent of March 6, 1846. Repertory of Patent Inventions, (E. S.,) ix, 46,] to produce chlorid of sulphur in the rubber paste, was led to perform the following experiments:

When flowers of sulphur and dry hypochlorite of lime, (bleaching powder,) are shaken together, a very strong odor of chlorid of sulphur is immediately developed. If the mixture be somewhat forcibly rubbed in a mortar, elevation of temperature ensues, the sulphur softens and the mixture becomes solid while abundant vapors are evolved. When a much larger amount of sulphur than of the hypochlorite is used and friction is avoided when the two are blended, a mixture is obtained which being added to the caoutchouc paste—either with or without the addition of inert matters, such as chalk, oxyd of zinc, etc., serving to give "body" to the product—effects the vulcanization of the latter either at the ordinary temperature, or when gently heated. By this means, objects of any thickness can be uniformly vulcanized.

If instead of employing an excess of sulphur, an excess of the hypochlorite be introduced into the mixture, and this be agitated, so much heat will be developed that the vessel containing the mixture can no longer be held in the hands; if the flask be closed, the action becomes so violent that the cork will be blown out, or the flask broken by a violent explosion.—*Comptes Rendus*, May, 1860, l, 876.

4. *Preparation of Cyanid of Barium, and of Ammonia with the Nitrogen of the Air;* by MARGUERITTE and DE SOURDEVAL.—In a brief preliminary note, the authors claim:—

That baryta when calcined in the presence of charcoal and of atmospheric air combines very readily with carbon and nitrogen, cyanid of barium being formed.

That the cyanid of barium heated in a current of aqueous vapor is decomposed at a temperature of about 300° (C.) [572° F.] and disengages in the form of ammonia, all the nitrogen which it contains.

Trials which they have made upon a tolerably large scale, have been eminently successful, leading them to hope that not only the various cyanids employed in the arts, but also ammonia and nitric acid may thus be economically produced.*—*Comptes Rendus*, June, 1860, l, 1100.

5. *Gun-Cotton Filters;* by Prof. BOETTGER.—Since gun-cotton—itself a product of the action of strong acids—when properly prepared is scarcely at all acted upon at the ordinary temperature, by chemicals, being capa-

* To prepare baryta from its carbonate, M. and De S. ignite a mixture of the latter with the pitch ("asphaltum") of coal tar. Each molecule of the carbonate being thus brought in contact with the reducing agent, carbon, excellent results are obtained, the decomposition of the carbonate being easy and the product of baryta abundant. From observing the oder of ammonia which was at times developed during their experiments upon this method of preparing baryta the authors were led to the important discoveries which are noticed in the text.—*Répertoire de Chimie appliq.* June, 1860, ii, pp. 169, 170.

ble of withstanding the most corrosive agents; it affords a material for filtering strong acids, and the like, as well as liquids which would be decomposed by contact with the organic matter of ordinary filters, the excellence of which cannot be too highly extolled.

Besides employing it for removing from strong nitric acid the chlorid of silver which is precipitated in the common method of purifying this acid by means of nitrate of silver, (as has recently been advised in the Berlin *polytechnisches Intelligenz-Blatt*, No. 4, p. 30); Boettger affirms that, he has for several years past, found it of special use in filtering off the slimy precipitate, containing selenium, which is gradually deposited, when fuming sulphuric acid is mixed with a little water; in separating crystallized chromic acid from the sulphuric acid of the mother liquor; and in filtering concentrated solutions of permanganate of potash, in order to separate suspended peroxyd of manganese. He has even found the gun-cotton well suited for filtering concentrated alkaline lyes, aqua-regia, and concentrated solutions of chlorid of zinc, not to allude to many other instances. In using the cotton a small bit of it is pushed loosely, like a stopper, into the throat of a funnel.

The materials which have heretofore been used for similar purposes, viz.: garnets, asbestos, powdered glass, &c., are very much inferior, as filters, to the loose, fibrous gun-cotton.—From *polytechnisches Notizblatt*, 1860, No. 7; in Dingler's *polyt. Journal*, clv, 463.

6. *Preservation of Flesh;* by VERDEIL.—Having been separated from the bones, and, as far as possible, from fat, the flesh is cut into slices from one to five centimetres, (one centimetre =0·3937 inch,) in thickness; the slices being cut as nearly as possible across the grain of the flesh. These are now laid upon hurdles of basket-work, which are subsequently placed in a chamber. As soon as a sufficient number of the trays have been introduced into the chamber, it is closed, and steam under a pressure of three or four atmospheres, consequently of 135° to 145° C. [=275° to 293° F.] is admitted through several openings.

The chamber, which may be of lead or iron, must not be absolutely tight, a small outlet for the steam being necessary, in order that the pressure may not become too great.

After from six to ten or fifteen minutes, according to the kind of flesh and the thickness of the slices, the steam is shut off, this part of the process being finished.

The flesh is now very nearly in the condition of boiled meat, but has retained all of its ingredients, the albumen having been coagulated: its taste recalling that of roasted meat. It presents a wrinkled appearance, is of a gray color, and may be readily divided.

Being removed from the steam chamber the flesh is now placed upon trays, or hung upon hooks, in another chamber which is warmed, but in which the temperature is never allowed to exceed 40° or 50° C. [=104° to 122 F.]. The drying process is completed in the course of eight or twelve hours.

Packed in tight casks or in tin boxes, so that it may be protected from the action of moisture, and from insects, the flesh thus prepared may be preserved for any length of time which may be desirable. It is nevertheless well to place a layer of salt in the casks, in order that it shall absorb any moisture which the flesh may have retained. Before using

this meat it must be soaked for an hour or two in warm water in which it softens and regains its original condition. When boiled with water it affords an excellent soup, and passes into a condition, in which it cannot be distinguished from fresh meat.—From *Le Génie Industriel;* in BOETTGER'S *polytechnisches Notizblatt*, 1860, xv, 71.

7. *Magnesium as a Source of Light.*—Prof. A. SCHMITT calls attention to the practicability of employing metallic magnesium for purposes of illumination, as had already been suggested by Bunsen. From the researches of the last named chemist, it is known that when magnesium is ignited it readily takes fire and burns with an exceedingly brilliant flame. The intensity of the light thus produced, as determined by Bunsen and Roscoe in one of their photo-chemical researches (*Pogg. Annalen*, cviii, 261, *et seq.*) is only some 525 times less than that of the sun. Compared with an ordinary candle, it appeared that a wire of magnesium 0·297 millimetre [1 mm. =0·0394 inch] in diameter, produced as much light in burning as 74 stearine candles, five to the pound. In order to support this light during one minute, a piece of wire 0·987 metres long, weighing 0·1204 gram [1 grm. =15·4325 grains], was required.

Only 72·2 grams of magnesium, therefore, would be needed, in order to maintain during ten hours an amount of light equal to that of 74 stearine candles, consuming about 10·000 grams of stearine.

According to Bunsen, magnesium wire is readily obtained, by forcibly pressing the metal through a hot steel die by means of a steel piston. Bunsen's arrangement for burning the wire was made, by connecting spools of it with rollers moved by clock-work so that the wire should be unrolled like the ribbon of paper in Morse's telegraph, the end of the wire thus gradually pushed forward, passed into the flame of an ordinary alcohol lamp, where it took fire.

It is evident that a magnesium lamp of this sort must be much simpler and more compendious than any of the existing arrangements of the electrical, or of Drummond's light; for light-houses, &c.: where an intensely brilliant illumination is required it can hardly fail to rival either of these. Where an extraordinary amount of light is needed, it could readily be produced by burning large wires, or several thin ones at the same time. Another important consideration is the fact that the spools of wire, as well as the clock-work and spirit lamp, are easily transportable.

It is not, however, to the intensity alone of the magnesium flame that these lamps owe their utility, for the photochemical, (*i. e.*, photographical) effect of the light is also very great. According to Bunsen, the photochemical power of the sun being only 36·6 times greater than that of the magnesium flame. The latter must therefore be useful in photographing by night or in any dark or subterranean locality; the evenness and remarkable tranquillity of the flame, especially commending it for this purpose.

The present high price of magnesium, it is true, must prevent any extended use of it for technical purposes. For example, Lenoir of Vienna charges 3 Florins, [1 Fl. =51 cts.] for a gram of it, hence the cost per minute of the light just described, would be 36 *Neukreutzer*, [1 ktr.= about $\frac{5}{8}$ of a ct.,] and the cost during ten hours, would amount to 216 Florins, while the ten kilogrammes of stearine could be procured for less than 14 Florins. But even at this price, it could still be used by photo-

graphers, since it would only be required for exceedingly short intervals of time, and all unnecessary consumption of the wire might be prevented by stopping the clock-work.—From *Stamm's Illustr. Zeitschrift*, 1859, p. 332; in *polytechnisches Notizblatt*, 1860, xv, 56.

8. *Method of employing carbonic acid in connection with the hypochlorite of lime used for bleaching paper-stock.*—An apparatus devised by FIRMIN DIDOT and BARRUEL of Paris, for introducing carbonic acid, prepared by burning charcoal, into the solution of hypochlorite of lime, (bleaching salt,) while the latter is in contact with the fibre which is to be bleached, is described in the Nov., (1859,) No. of Barreswil's *Répertoire de Chimie*, Appliquée, vol. i, p. 457.

The carbonic acid on being introduced into the solution of bleaching salt, unites with the lime, thus setting free hypochlorous acid, the decolorizing action of which is infinitely more energetic when it is at liberty than when in combination with a base.

This process, says Barreswil, is of extreme simplicity, and one is at a loss to comprehend why it had not been sooner invented, in view of the fact that each and all of its phases have been so long and so well known.

In order to judge of the practicability of the new process—in so far as concerns difference of price, strength, and whiteness of the paper, and the duration of the operations in the two systems, (new and old,) of bleaching—comparative experiments were instituted, by the Messrs. Firmin Didot, upon carefully assorted rags. The cost of the chemicals and labor, and the amount of time required, having been exactly noted. After bleaching, the pulp was converted into paper. The different papers were then carefully tested. As the result of these experiments, it appeared that the new process was more energetic and more rapid than the old method, *au chlore*, [*chlorure?*] *liquide*, [with solution of bleaching salt,] and that in many cases it is also equally energetic with the process in which chlorine gas is employed. Over the latter it has the advantage of not destroying to so great an extent, the fibre of the pulp.

Since the details of the process, which for that matter consists merely of arrangements for thoroughly washing and cleansing the carbonic acid employed—the latter being then introduced into the bleaching vats, just as if it were steam, through coils of pipe pierced with holes, which are placed at the bottom of the vats,—cannot well be explained without a diagram, we must refer the reader who may desire these to the original article, in which the apparatus is figured. F. H. S.

9. *New "fusible metal."*—Dr. B. WOOD of Nashville, Tenn., has secured a patent (Weekly Scientific Artizan, Cincinnati, May 5th, 1860,) for an alloy composed of cadmium, tin, lead and bismuth, which fuses at a temperature between 150° and 160° F. The constituents of this *fusible* metal may be varied according to the other desired qualities of the alloy—viz: cadmium one to two parts; bismuth seven to eight parts; tin two parts; lead four parts. It is recommended as being especially adapted for all light castings requiring a more fusible material than Rose's or Newton's "fusible metal," it having the advantage of fusing at more than 40° F. lower temperature than these alloys, and owing to this property may replace many castings heretofore made only with amalgams. Its fusing point may be lowered to any extent by the addition of mercury, which

may be employed within certain limits without materially impairing the tenacity of the metal. In a letter to the Editors, dated Nashville, June 9th, 1860, Dr. Wood says:—

"One point in particular that strikes me as being worthy of note is the remarkable degree in which Cadmium possesses the property of promoting fusibility in these combinations. The alloy of one to two parts *cadmium*, two parts lead, and four parts tin is considerably more fusible than an alloy of one or two parts *bismuth*, two parts lead and four parts tin; and when the lead and tin are in larger proportion the effect is still more marked. It takes *less* cadmium to reduce the melting point a certain number of degrees than it requires of bismuth, besides that the former does not impair the tenacity and malleability of the alloy, but increases its hardness and general strength.

Bismuth has always held a pre-eminent rank among metals as a fluidifying agent in alloys. Its remarkable property of 'promoting fusibility' is specially noted in all our works on chemistry. But I do not find it intimated in any that cadmium ever manifests a similar property. The fact indeed appears to have been wholly overlooked—owing perhaps to the circumstance that as an alloy with certain metals cadmium does not promote fusibility.

Cadmium promotes the fusibility of some metals, as copper, tin, lead, bismuth, while it does not promote the fusibility of others, as silver, antimony, mercury, &c., (i. e., does not lower the melting point beyond the mean.) Its alloy with lead and tin in any proportion and with silver and mercury, within a certain limit, say equal parts and especially of two parts silver and one of cadmium or two parts cadmium and one mercury are used, are tenaceous and malleable, while its alloys with some *malleable* metals, (gold, copper, platinum, &c.,) and probably with all brittle metals *are* 'brittle.'

I notice a great discrepancy among authors as to the melting point of this metal. It is usually put down the same as that of tin, (442° F.) Brande (*Dict. of Science and Arts,*) says it 'fuses and volatalizes at a temperature a little below that at which tin melts.' Daniell, (according to the *New American Cyclopedia,*) gives its melting point at 360° F., while Overman places it at 550° and gives 600° as the temperature at which it volatalizes.

The latter is doubtless the nearest the truth. The metal requires for its fusion a temperature too high for measurement by the mercurial thermometer, but from relative tests with other metals I should place its melting point in round numbers at 600° F. as it melts and congeals nearly synchronously with lead, the melting point of which is stated by different authorities as 594°, 600°, and 612° F. It volatalizes at a somewhat higher heat.

I draw attention to these facts believing that the metal possesses properties valuable to Art and interesting to Science, and that it merits more thorough investigation than appears to have been bestowed upon it."

[We have had time only to repeat a few of Dr. Wood's interesting experiments in regard to the remarkable influence which cadmium exercises in lowering the fusible point of various alloys. The alloy made by fusing together two parts of cadmium, two parts tin, four parts lead and eight parts bismuth melts at a temperature varying not far from 70° C. (158° F.) It may appropriately be called "*Wood's fusible metal.*"—Eds.]

II. GEOLOGY.

1. *Note from* Dr. NEWBERRY, *in reply to* Mr. LESQUEREUX, (in a letter to the Editors).—I see by the note from Mr. Lesquereux, [contained in this Journal, xxix, 435,] that my letter from Santa Fé was unacceptable to him. This both surprises and grieves me, as the thought that he might be drawn into the controversy had not occurred to me; and I am sincerely sorry to learn that one with whom I have had so many years of friendly intercourse could so readily misconstrue both the statements and the spirit of my letter. Possibly its tone may have failed to reflect the great respect which I have had and still have for Prof. Heer; and to others than Mr. Lesquereux it may have seemed not altogether courteous. It should be borne in mind, however, that the discussion in reference to these fossil plants, and the age of the strata containing them, had already been repeatedly brought before the public; and that in this discussion the tone of the associate of Prof. Heer had been marked by a degree of arrogance difficult to bear patiently. Prof. Heer had called them *Miocene*—an error which with the imperfect material in his hands was natural enough; and one which should detract nothing from his high reputation—but by the testimony of several observers they had been proved *Cretaceous*. Ignoring their testimony, however, and adhering to his former opinion, a portion of his letter to Mr. Lesquereux was written to perpetuate what I knew to be a mistake. It also did me, as I conceive, manifest injustice. That letter reached me when I had been for months in exile, and where I was surrounded by proofs of the truth of the position I had before taken—circumstances favorable to the development of a little honest indignation. In the freshness of that feeling my reply was written, and I am willing to admit, if others think so, that it was not sufficiently respectful.

So much for the manner of my letter. In regard to its statements of fact I fear I shall be unable to make any such concession. On the contrary, my regard for truth requires that I should repeat each and all of them.

(1.) Prof. H. considered the plants in question Miocene. There is not the shadow of a doubt that they are Lower Cretaceous.

(2.) Prof. H. states that "*except Credneria* and *Ettingshausenia all the genera enumerated* (in my list) *are represented in the Tertiary and not in the Cretaceous.*" It will be observed that he does not say they are *characteristic* of the Tertiary, or "of the Tertiary," as Mr. Lesquereux quotes him—but distinctly affirms that they *are not represented in the Cretaceous.*" Hence there is no propriety in the remarks of Mr. Lesquereux on this point; and the error in the statement of Prof. Heer shown by reference to Stichler's paper, before quoted, remains unexplained. If that error was not accidental, it was designed. If accidental, as I cannot for a moment doubt, the offensive clause of my letter is no more than just. If designed, stronger language would be admissible. The appeal to "authority" has been nearly exhausted in this discussion, and the time has passed when personal influence could make errors pass for truths. Prof. Heer is a man of estimable character, of

great learning and of world-wide reputation, and, I am sure, would be one of the last to ask us to believe a scientific statement simply because he had made it.

(3.) In my letter I made no supposition in reference to the Tertiary flora of Kentucky, Tennessee or Mississippi. I merely stated some facts in reference to the Miocence flora of the country bordering the Upper Missouri, 1000 miles distant from the nearest of these States. I also distinctly said that the absence of tropical plants from the collections made there, was only negative evidence. They may be found in that region to-morrow, but at the time of writing that letter they had not been found, and all the material in my hands indicates, as I then said, a Tertiary climate warmer than the present, but still temperate.

(4.) I was also fully aware that marine Tertiary deposits extend up the Mississippi even higher than stated by Mr. Lesquereux. I excluded them from "*the central portion of the continent*;" by this meaning, as I then explained, the region between the Mississippi and the Sierra Nevada. Here, too, the evidence is negative, but now stands just as I represented it.

(5.) Mr. Lesquereux says: "I cannot admit, as Dr. Newberry appears to do, that the fossil flora of the American Cretaceous, ought to be closely related to the European." My only reference to this question will be found on page 216 (Journal, March, 1860), where I say—"We may find hereafter, in other parts of the continent than those in which I have examined the Cretaceous strata, fossils which shall assimilate our flora of that period more closely to that of Europe, but, so far as at present known, our plants of this age present an *ensemble* quite different."

(6.) The statement made by Mr. Lesquereux that "the age of the strata from which American fossil plants have been taken is mostly uncertain," is manifestly incorrect. At least nine-tenths of the species enumerated are from the Carboniferous and Devonian rocks, whose place in the geological series is certainly well ascertained. Of those collected and not yet catalogued, perhaps an equal proportion have been obtained from the Cretaceous and Miocene strata, of which the places in the series have been as accurately determined, by the molluscous fossils which they contain.

(7.) It is true that in America fossil botany has had but few devotees, and doubtless all of them have at times keenly felt the want of more books and specimens bearing on their subjects of study. Still, I believe everything that has been published in reference to fossil plants is accessible to the American student within the limits of his own country. At the same time it is also true that a satisfactory comparison between the extinct floræ of Europe and America can only be made by means of full collections of well-marked *specimens*, many more than we yet possess.

Mr. Lesquereux is aware, as is every one who has given the subject any attention, that our knowledge of the floræ of the different geological formations has been limited, not so much by the want of learning and acuteness in the cultivators of fossil botany, as by the small number and imperfect preservation of the fossil plants collected. It could hardly

be otherwise, then, than that in the whole New World material should be discovered which should throw new light on the ancient vegetation of the globe. The idea that no American can be qualified to make good use of such material, is another instance of the arrogance to which I have before alluded, and to which it would be unmanly tamely to submit.

III. BOTANY AND ZOOLOGY.

1. *Geological and Natural History Survey of North Carolina. Part III. Botany; containing a Catalogue of the Plants of the State, with Descriptions and History of the Trees, Shrubs, and Vines.* By Rev. M. A. CURTIS, D.D. Raleigh, 1860: the First part only, the *Woody Plants of North Carolina.* pp. 123, 8vo.—We have turned over the pages of this popular exposition with much interest, and gleaned some valuable information. "Botanists will of course find fault with it," says the author, who we well know could write scientifically and profoundly enough, if he so pleased, but who has here come down to the level of his most unlearned readers, discoursed separately of trees, shrubs, and vines, and classified these in a fashion which might well shock the susceptibilities of a stickler for technical nomenclature and natural system in botany. Now, we are not shocked at all; indeed we quite enjoy a glimpse of Flora *en deshabille* and slip-shod, and are well aware how much easier it is, and how much better in such cases, to fit your book to its proper readers than to fit the readers to it. The fault we should find is not with the plan of this Report, but with the quantity. We could wish for more of it, for a volume as large at least as Mr. Emerson's Report on the Trees and Shrubs of Massachusetts. We quite like to see the popular names put foremost, but would suggest that the botanist who does this should *lead* as well as follow the indigenous nomenclature, so far as to correct absurd or incongruous local names and introduce right or fitting ones as far as practicable. For instance *Virgin's Bower* is not a proper name for *Wistaria frutescens*, and is rightly applied to *Clematis Virginiana* over the leaf. (We venture to add, in passing, *C. Viorna* to the list, having gathered it in Ashe County.) And, although the people along shore call *Baccharis* by the name of the English annual weed, *Groundsel*, it were better to write it *Groundsel-tree*. *Yellow wood* is the name of *Cladrastis*, rather than of *Symplocos*, which the Carolinians call *Horse-Sugar*. Dr. Curtis can coin a name upon occasion; for surely nobody in Carolina knows *Menziesia globularis* as *False Heath*, nor has it any scientific claim to this appellation. While in critical mood we may express a strong dissent from the proposition that *Rhododendron punctatum* is too inferior to the other two species "to attract or deserve much attention." With us, it is surpassingly beautiful in cultivation, none the less so because its habit is so different, having light and pendent branches, when well grown forming broad and thick masses, and loaded with its handsome rose-colored blossoms. While *Leucothoë Catesbæi* is called "a very pretty shrub," the far handsomer *Andromeda floribunda*, so much prized by our nurserymen, gets no commendation. *Magnolia Fraseri* may not only be "cultivated in the open air near Philadelphia," but is perfectly hardy near Boston, and the earliest to blossom; but we never noticed the fragrance of the flow-

ers. On the other hand, as it is native as far south as Florida, it might thrive in plantations any where in North Carolina. The flowers of *M. cordata* are described as if larger than those of *M. Fraseri*, instead of the contrary; we could hardly say much for their beauty, except in comparison with those of the common Cucumber-tree. *Prunus Virginiana* is omitted; yet surely it is not wanting in North Carolina. And it is almost an excess of conscientiousness to leave out *Cladrastis*, the handsomest tree of the country, all things considered, when it is known to grow only a few rods over the Tennessee line.

On the other hand, we are disposed to doubt if the genuine *White Spruce*, (*Abies alba*,) occurs in North Carolina. At length we know this tree, but only in Canada and parts adjacent. It is more, instead of less northern in its range than *A. nigra*. But since President Wheeler has pretty nearly determined the existence of *A. Fraseri* on the Green Mountains in Vermont, we could not deny that *A. alba* grows with the latter on the high mountains of North Carolina. We make our little criticisms freely,—as we know the excellent author would wish,—for we think it likely that this part of the Report will pass to a second edition,—when we hope it will be largely augmented. A. G.

2. *Thwaites, Enumeratio Plantarum Zeylanicæ*, Parts I. II. 8vo, pp. 160. 1858–1859.—A complete enumeration of the known plants of Ceylon, with characters of new or little known genera and species, and numerous descriptions and critical remarks, the synonymy, &c., elaborated by Dr. Hooker. These two published parts extend from *Ranunculaceæ* to *Compositæ*; so that a good-sized volume will complete the work, and constitute an important adjunct to the great Indian Flora. A. G.

3. *Walpers, Annales Botanices Systematicæ*, continued by Dr. C. Müller, Berol.—Five parts of the fifth volume are published, extending to page 800, and to the order *Coniferæ*. A. G.

4. *Bueck, Index ad De Cand. Prodromum, etc.* Pars III. Hamburg, 1859. pp. 506.—This useful Index to De Candolle's Prodromus is here continued from the second part of the seventh to the thirteenth volume. As we may expect that at no distant period the Prodromus will be terminated, as announced, we trust that the next Index will combine the whole into one continuous alphabetical list. A. G.

5. *Synopsis Methodica Lichenum omnium hucusque cognitorum, præmissa introductione lingua Gallica tractata, scripsit* William Nylander. Fasciculus II, Parisiis ex typis L. Martinet via dicta Mignon, 2, 1860. 8vo. pp. 141–430.—We are glad to welcome another portion of the important work of Dr. Nylander, which is indispensable to every Botanical library. Beginning with the *Caliciei*, the present part embraces the *Bæomycei*, the *Cladonici*, the *Usnei*, and the *Parmeliei*, ending with the genus *Physcia*. The higher tribes of Lichens are by no means the least difficult, and nothing in the part before us is more acceptable than the author's elaboration of the genus *Sticta*;—disposed by him in *Sticta*, *Stictina*, and *Ricasolia*. The last general synopsis of Lichens, that of Acharius, was published nearly fifty years ago, and the vast amount of valuable matter, scattered in many publications, which has since been accumulating, has long needed to be brought together in one work. This Dr. Nylander proposes to accomplish, adding also the results of his investigation of all the

most important collections; and disposing the whole in a system, which, while it aims to retain all that was most valuable in the old, gives us, as it should, the whole light of modern (microscopical) science upon both old and new. We have only room to add here that "*Usnea lacunosa, Willd.*," is a name found only in Willdenow's herbarium, and was anticipated in print by *U. cavernosa*, published by the present writer, in the appendix to Agassiz's tour to Lake Superior. Messrs. Westermann and Company of New York, will receive subscriptions to the synopsis, which is put at a moderate price for so handsome a book. E. T.

6. *Reports of Explorations and Surveys* to ascertain the most practicable and economical route for a Railroad from the Mississippi River to the Pacific Ocean, made under the direction of the Secretary of War, in 1853–6, &c., vol. x. Washington, 1859.—FISHES; by CHARLES GIRARD, M.D. Washington, D. C., 1858.

Of recent contributions to our knowledge of special Faunas, none have been of greater importance or interest than the report on the Fishes of Western North America. In this volume, Dr. Girard has incorporated almost everything known to the date of publication concerning the Ichthyology of our Pacific possessions.

In the introductory remarks, (which with some variations, are duplicated) a general view is given of the Piscine Fauna of Western North America. The families which are richest in genera and species, and which are most characteristic of the Californian Fauna especially, are those of the Cataphracti, the Blennoids, the Embiotocoids, and the Pleuronectoids. Of all these families, many new genera, previously indicated in the "Proceedings of the Academy of Natural Science of Philadelphia," are described and illustrated.

The classification of the late Johannes Muller is adopted, and the orders, suborders and families of which representatives are described, are all characterized; the genus *Amblodon*, however, is retained in the family of Sciænoids; this should in strict accordance with the principles of that classification, be transferred to the order of Pharyngognathi. Agassiz has demonstrated the union of the lower pharyngeal bones, the only character on which the order depends. Dr. Girard does not appear to have noticed this discovery as he has not adverted to it in the generic diagnosis of *Amblodon*.

Of the family of Percoids, representatives of only one genus are yet known as inhabitants of the Pacific coast. This genus has been described as new under the name of *Paralabrax*, and includes two species, both of which had been first referred to *Labrax* in the Proceedings of the Academy. It is here placed in the vicinity of *Serranus*.

Many fresh water Percoids are described, belonging to the genera *Dioplites* Raf., *Pomoxis* Raf., *Ambloplites* Raf., *Calliurus* Raf., *Bryttus* Val., *Pomotis* Raf., *Labrax* Cuv., and *Stizostedion* Raf. The species described have been collected in many distant places west of the Mississippi river, but one species (*Ambloplites interruptus*) having been obtained in California.

The name of *Dioplites* has been substituted for *Grystes* of Cuvier. It would by many have been deemed more proper to have applied Rafinesque's generic name of *Lepomis*. Rafinesque in his "Ichthyologia Ohi-

ensis" has characterized the genus *Lepomis* and divided it into two subgenera, *Aplites* and *Dioplites*, giving at the same time, to each of the species, the generic name of *Lepomis*—*Aplites* and *Dioplites* having been separated in consequence of an error of observation, and not differing from each other, cannot be retained—*Lepomis* must be therefore used, as it should have been for one of the genera or subgenera, if both had been established on true principles. *Dioplites* is restricted by Dr. Girard to the species without teeth on the tongue.

The Cuvieran section of "Percoids a Joues cuuirassés" is retained as a "tribe" under the name of Cataphracti, and is divided into three families, *Heterolepidæ*, *Cottidæ*, *Scorpænidæ*. Perhaps the families so indicated are valid, but the characters given to them are vague and will require revision after a comparative study of the foreign genera.

The family called Heterolepidæ had been previously named by Swainson Chiridæ, and that name should have been retained, as well on account of its priority, as its consonance with the terminology of the other families.

In the family of Cottoids, the species are distributed into ten genera, all of which appear to be founded on good characters, but the names of some of which are objectionable.

Among the Salmonoids, the three genera of Valenciennes, *Salmo*, *Fario* and *Salar* are accepted, but we notice that Dr. Girard has named all the new species he describes as belonging to those genera, Salmo—"*Grd. MSS.*" in the synonymical lists of the species.

In the family of Clupeoids, the modifications of Valenciennes have not been adopted, and the genus *Hyodon* is interposed between *Meletta* Val. and *Engraulis*.

The genus *Anarrhichthys* of Ayres is adopted; its only species is called *Anarrhichthys felis* Girard. To this name we desire to draw the attention of our readers, as an important question of nomenclature is involved.

In the "Proceedings of the Academy of Natural Sciences of Philadelphia" for 1854, Dr. Girard mentions a fish which he calls *Anarrhicas felis*, and observes that two large specimens were received in such a precarious state of preservation that there was no probability of keeping them, and that having mislaid the notes, *no diagnosis could be given*.

Subsequently, Mr. Ayres, in the Proceedings of the Californian Academy, gave a full description of a species which he referred to a new genus and called *Anarrhichthys ocellatus*, which was chiefly distinguished from *Anarrhicas* by its anguilliform body, and the union of the dorsal, caudal, and anal fins. This is the species that Dr. Girard has claimed as his own *Anarrhicas felis*.

To this reference we would remark that as *Anarrhicas* has by all modern naturalists, been restricted to such species as had the dorsal and anal fins separated from the caudal, we would infer that any species placed without comment in the genus would have those characters. Dr. Girard's name of *Anarrhicas felis* was not only without any description whatever and therefore not established, but a statement by implication was made that the species possessed the stout body and fins of *Anarrhicas*, and was consequently in direct opposition to the characters of *Anarrhichthys ocellatus*. Such being the case, Dr. Girard's name cannot be adopted, and that of Ayres must be retained.

The family of *Golidæ* is limited to the species with the ventral fins united in the manner of a funnel and thus excludes the *Eleotroids* as well as the *Cyclopteroids*. The latter are very properly regarded as distinct, but the propriety of excluding the former from the *Gobioids* is more doubtful.

The genus *Gobius* is limited to the species with cycloid scales. The name cannot be retained for such species, as the genus had by several naturalists been previously restricted to species with pectinated scales. Mr. Gill has framed for the two species thus referred to *Gobius*, a genus which he has called *Lepidogobius*.

We find that in the family of Cyclopteroids, the Gronovian name of *Cyclogaster* is substituted for the Artedian name of *Liparis*. *Liparis* has been almost universally accepted by naturalists, and being the prior name, should be adopted. No description is given by Artedi of the generic or specific characters of *Liparis*, but the references he has given are full and ample, enabling us without doubt to ascertain what is meant.

In the Proceedings of the Academy of Natural Sciences, Dr. Girard has described a genus under the name of *Homalopomus* which he has referred to the Trachinoids. This is now referred to the Gadoids, and a doubt is even expressed whether it is distinct from *Merlangus* or *Merlucius*. It does not appear to differ from the latter genus. The cause of the former erroneous reference is attributed to the broken tips of the rays of the specimen on which the species was founded.

The family of Embiotocoids is rich in generic forms, nine being described and illustrated, and references being made to the descriptions by Dr. Gibbons of five others which Dr. Girard was unable to identify with his. Some of the species described as new by Dr. Girard will probably be found to have been previously indicated by Gibbons.

In the remarks on the family, no mention is made of the presence of the two rows of lamellæ which are present on each of the branchial arches, and which was especially noticed by Prof. Agassiz. This character has been regarded as having considerable importance by many ichthyologists and being one of the best distinctive ones of the family, the fourth branchial arch of the Labroids having but one row of lamellæ.

Dr. Girard claims "that the real knowledge of the remarkable peculiarities concerning some of their habits was obtained in the spring of 1852, by Dr. Thomas H. Webb, while attached to the United States and Mexican Boundary Commission."

The genera described and illustrated are *Embiotoca* Ag. with seven species, *Damalichthys* Girard, *Phanerodon* Grd., *Abeona* Grd., *Rhacochilus* Ag., *Hysterocarpus* Gibbon, *Holconotus* Ag., each with one species, *Ennichthys* Grd., and *Amphistichus* Ag., each with two species.

These genera are chiefly characterized by the comparative size of the head; the character of the lips, and the attachment or non-attachment of the lower one by a frænum to the jaw, the comparative protractility of the premaxillaries; the number of rows of teeth on the jaws, and the outline of the dorsal fin.

Dr. Girard has given some information on the embryology of the Embiotocoids, which will prove of general interest. He denies the presence of any resemblance between their gestation and that of the marsu-

pial mammals. Want of space forbids us to make extracts from the recorded observations, and we must remain satisfied with referring to the text and plates of the report.

Of the Cyprinoids, numerous genera and species are described from almost every portion of the west. The family is divided into the tribes of 1. *Cyprini* with teeth of the molar kind, of the grinding type, 2. *Catastomi*, with pectiniform teeth; 3. *Chondrostomi*, in American species of which there are no barbels, and the teeth are of "the grinding type and cultriform kind;" 4. *Pognichthyi*, in which barbels are present, the snout is prominent, and the pharyngeal teeth are more or less hooked; and 5. *Alburni*, which differ from the *Pogonichthyi* by the absence of barbels.

The author has devoted much time and research to these groups, and all of the genera may be good, but some of them appear to be distinguished on quite slight grounds, and many of the species are small and perhaps the young of others. But on this question we will not venture to disagree with Dr. Girard.

Most of the new genera have received Indian names, and although not cacophonous, some of those names appear strange to ears that have been mostly accustomed to Latin and Greek derivatives.

In *Dionda*, a genus of Cyprinoids, Dr. Girard has named two species collected by Capt. John Pope, *Dionda episcopa* and *D. Papalis*.

The genus "*Argyreus* Heckel" is synonymous with *Rhinichthys* of Agassiz. It is not probable that Dr. Girard will be sustained by Ichthyologists in this application of Heckel's name. An extract from the remarks of the learned Doctor himself on the nomenclature of the genus will show the history of the name *Argyreus*.

"Heckel includes in this genus two species which are generically distinct *Cyprinus atronasus* Mitch., and *Cypr. rubripinnus* Mus. Par. MS. But *Cyprinus rubripinnis* is identical with *Leuciscus cornutus*; and since *Leuciscus cornutus* is to enter the genus *Plargyrus* of Rafinesque, *Cyprinus atronasus* remains as the type of the genus *Argyreus* which again is identical with *Rhinichthys*. It must be recollected, however, that the teeth figured by Heckel under the name of *Argyreus rubripinnis* are those of *Plargyrus cornutus*."

The diagnosis and illustration of *Argyreus* having been by Heckel founded on "*Cyprinus rubripinnis*" and not answering to *C. atronasus*," the former species in the type of the genus, and if Rafinesque's name *Plargyrus* is adopted, Heckel's name must be treated as a synonyme. Heckel would scarcely have referred a species of *Rhinichthys* to the same genus as *C. rubripinnis* if he had known the pharyngeal teeth, and he could only have done so from an ignorance of the species. Because a species on which a genus is established belongs to a previously named genus, it by no means follows that the generic name has to be used for another species of the genus, when it proves to be distinct from the type.

If the above views are correct, *Rhinichthys* will have to be retained for the genus called by Girard *Argyreus*, and the two species described by him in his report must be called *Rhinichthys dulcis* and *R. nubilus*.

The Lepidostei have been separated apparently on good grounds into three groups distinguished by the comparative length and breadth of the

snout, and the presence of one or two rows of teeth in the upper jaw. For two of these groups, Rafinesque's names of *Cylindrosteus* and *Atractosteus* have been adopted, although to Dr. Girard, the credit of first giving them valid characters is due.

In the Plagiostomes, we have some interesting additions. A second species of *Triakis* (*T. semifasciatus* Grd.) is described. Probably this is the species noticed as *Triakis californica* in the list of Chondropterygii of the British Museum, but as this name is unaccompanied by a description, Girard's name must of course be retained. A new species of *Heterodontus* is described which is called *Cestracion francisci* Grd. But the name *Heterodontus* of Blainville must be retained for the Cuvieran *Cestracion-tes*, and the species must be consequently called *Heterodontus francisci*. The *Notorhynchus maculatus* of Ayres is referred to the genus *Heptanchus*, Raf.

Among the Rays, a second species of Muller and Henle's genus *Uraptera* is made known.

Among the Cyclostomi we perceive that Dr. Girard has not only retained the genus *Ammocœtes*, but he has even separated from it a new genus which he has called *Scolecosoma*. The researches of A. Muller have demonstrated that the *Ammocœtes* are only the young of Petromizontoids, and there is no reason to doubt his accuracy. Dr. Girard must have been acquainted with these researches, and it would therefore have been more advisable not to have added to the number of names, until it was certain, as may possibly be the case, that there are fishes of the *Ammocœtoid* type which are adult.

We have now concluded, and although we think that there is cause to dissent from the author in many cases, we most cheerfully bear witness to to the general ability with which the work has been performed, and to the very great advance in our knowledge of the Fauna of our Pacific possessions.

To the Smithsonian Institution, we are indebted for the accumulation of the materials which have been used in the elaboration of the report. Although published by the liberality of the General Government, it is one of the valuable "contributions to knowledge" which we owe to the fostering care of that Institution. T. G.

IV. ASTRONOMY AND METEOROLOGY.

Solar Eclipse of July 18, 1860.—From the numerous accounts which have reached us of the observations made upon this Eclipse we place the following before our readers.*

1. *Notice of the Astronomical Expedition to Cape Chudleigh* (or *Chidley*), *Labrador*, (in a letter to one of the Editors).—*Dear Sir*:—The American Astronomical Expedition despatched to the coast of Labrador for the purpose of observing the total solar eclipse of July 17th, (astr. time), 1860, sailed from the Navy Yard at Brooklyn on the morning of the 28th of June last. The expedition had been organized by the accomplished and energetic Superintendent of the U. S. Coast Survey, under

* For LeVerrier's account of the French Expedition, see *Postscript*, p. 309.

authority of joint resolutions of Congress passed at the late session; and consisted of the following gentlemen, viz: Prof. Stephen Alexander, of the College of New Jersey, Prest. F. A. P. Barnard, of the University of Mississippi, Lieut. E. D. Ashe, R. N., director of the Quebec Observatory, Prof. C. S. Venable, of the College of S. Carolina, and Prof. A. W. Smith, of the U. S. Naval Academy at Annapolis. With these were associated the Commander of the Coast Survey steamer *Bibb*, conveying the expedition, Lt. Alexander Murray, U. S. N. Messrs. P. C. Duchochois, of New York, and J. P. Thompson, of the Coast Survey service, accompanied the corps for the purpose of taking photographic impressions of the eclipse; and Mr. W. A. Henry, of Washington City, attended as assistant to the chief of the corps.

In addition to the purely astronomical objects of the expedition, advantage was taken of the opportunity it presented, to secure determinations of the important magnetic elements at the observing station, as well as meteorological and other observations continued throughout the entire period of absence. The thermometrical and barometrical observations were kept up hourly from the time of sailing until the morning of the day on which the Bibb entered the harbor of Newport. On the day of the eclipse the intervals were reduced to a half hour. Frequent record was made of the surface and deep sea temperature of the water.

The magnetic observations were placed under the charge of Messrs. Edward Goodfellow and Samuel Walker, of the Coast Survey. The meteorological, under that of Prof. Venable, of the astronomical corps, assisted by Oscar M. Lieber, Esq., of South Carolina. Mr. Lieber also undertook such observations of the geology of the coast as the opportunities afforded would permit.

The history of the expedition and its results have been communicated by Prof. Alexander, the chief of the corps, to the Supt. of the Coast Survey, and will probably be, at a suitable time, presented to the public in full. The present memorandum is furnished by request, and with the permission of the Superintendent, with the view of presenting in concise form the facts of most immediate interest to the scientific public.

The track of the central eclipse left the eastern coast of Labrador in lat. 59° 51½′. On the evening of the 13th July, the expedition had reached this parallel; and was, for several hours, engaged in the endeavor to penetrate the seemingly unbroken and gloomily frowning barrier of precipitous cliffs which marked this rock-bound shore. The navigation at this point was of the most hazardous character. It was necessary to feel every foot of progress with the lead, and the successive soundings, which gave everywhere, a rocky bottom, showed the most singular and sudden variations of depth. One cast, for instance, would give ten fathoms, the next, no bottom with nineteen, and the next again, seven or eight fathoms. On every hand were seen rocky islets, rocks nearly submerged, or reefs and breakers.

Through such a sea, for five or six hours, from six o'clock till nearly 12 at night, the Bibb was engaged in cautiously seeking out for herself a harbor of refuge; and just about at the moment when the sun was passing his lower culmination, though a bright twilight still filled the atmosphere, she dropped her anchor in the inlet which divides Au-

lezavick Island from the main land of Labrador. This was the point which had been previously assumed to be fittest for the purposes of the expedition; but so imperfect and even erroneous had all the charts of the coast been found to be, that even the existence of the island was regarded as doubtful; and the identity of the inlet was not fully recognized until further celestial observation and some hydrographic exploration and survey had established it.

During Saturday, July 14, a location was fixed on for the encampment, and some progress was made in the shore arrangements. The harbor, once entered, proved to be commodious and secure; and it was found practicable to moor the *Bibb* so near to the land as to enable a portion of the party to sleep on board of her, and thus to avoid some of the discomforts of camp life, which, in a region so bleak and dismal, are by no means trifling. Others, including the meteorologists, the magnetic observers, and the members of the astronomical corps in charge of the transit instrument, were compelled to make a larger sacrifice of their ease. What that amounted to may be appreciated, when it is mentioned that several of the tents were blown down almost immediately after their erection; and that a wind as disagreeable for its piercing chilliness as for its force prevailed with little intermission during the entire stay of the expedition, amounting to eleven days. The tents when reërected, were secured, or anchored, by piling rocks upon the margin of the canvass.

The exact latitude of the observing station was a few seconds short of 59° 48′; the longitude, by chronometer, $4^h\ 16^m\ 53^s$ west from Greenwich.

Hardly had the landing been effected when there commenced a storm of wind and rain which rendered any attempt to use the transit instrument impossible for two or three days. Even up to the Tuesday night preceding the eclipse, the clouds had been so persistent as almost wholly to prevent the observation of the stars. On that evening the sunset seemed to hold out better promise for the coming night and the ensuing day. But on Wednesday morning the prospect for the day was more than doubtful. Fleecy cumulus clouds made their appearance in numbers constantly increasing; and at the moment when the eclipse began, the sky was more than half covered. The sun was, however, totally unobscured at the beginning and for a large portion of the time during the progress of the eclipse. Clouds were, however, continually drifting over it, concealing it for brief intervals entirely from view. At the very close, the alternations of sun and shade were so rapid, that it became a question of doubt whether the final contact would be secured, and a flutter of excitement prevailed throughout the observing corps: but the last four or five seconds presented the sun's eastern limb entirely unobscured; and the desired point was satisfactorily gained.

Just previously to the time of the sun's total immersion, a thin veil of cloud intervened between it and the observers, not dense enough to intercept the direct rays of the luminary, but too dense to allow the corona surrounding the dark moon during total obscuration to be visible. Lt. Ashe was fortunate enough, however, to catch one point of brightness and to fix its position in this corona; and this may serve a useful purpose hereafter, in corroborating observations elsewhere made,

under more favorable circumstances, in regard to the features of this beautiful and rare phenomenon. The bright point observed by Lt. Ashe was white and not ruddy. The expedition are unhappily unable to bear any testimony in regard to the roseate clouds which have been so often seen during total eclipses upon the moon's border. This has been a subject of great regret, the more so because the corona which was lost to the astronomical party, was not lost to such of the ship's company as remained on board of the Bibb. From the description given of it by these, the chief of the corps has prepared a drawing, in which there appear four principal radiant beams extending outward beyond the general limit of the coronal luminosity, in positions sufficiently well fixed to admit of comparison with observations made elsewhere. But as this class of observers were not furnished with instruments, they gave no testimony as to the positions, or even as to the presence of rosy clouds.

The whole astronomical corps observed the breaking up of the last line of solar light lingering before total obscuration, into the fragments commonly called "Baily's Beads," from Francis Baily, President of the Royal Astronomical Society, by whom they were described in the Mem. Astr. Soc. for 1837, as observed by him in the annular eclipse of 1836. These fragments were very evanescent, and were not preceded by those longer dark filaments or ligaments noticed by Mr. Baily on the same occasion, and more or less perfectly by others since. At the emergence of the sun, the beads were not noticed, owing probably to the veil of clouds. Only two of the observers attempted, in fact, to fix the exact second of emergence.

The darkness which prevailed during total obscuration was not as remarkable as had been anticipated by most of the observers. The present writer, for instance, found no difficulty in making pencil notes at this time, or in reading lines written in pencil in other parts of his note book. It was not necessary to bring the book nearer to the eye than usual.

The pallor or ghastly appearance which has been remarked at such moments in the human countenance by former observers, did not strike the members of this party, though it was looked for.

There was something indeed about the character of the gloom which was unusual and impressive, but it scarcely effected the tints of objects or rendered the face of nature very different from what it appears during early twilight. Clouds covered at the time almost nine-tenths of the heavens; and in the intervals of the clouds the blue of the sky was intensely deep and dark. On one side only was the horizon unobscured. This was on the north where the harbor opened out to the sea; and in this direction a beautiful rose and orange flush presented itself.

It would be easy to extend this notice to much larger dimensions, if space in the present number of the Journal was available.

The instruments employed in the astronomical observations, were a thirty-inch transit by Fitz, a fifty-one inch Fraunhöfer achromatic belonging to Princeton College, a forty-two inch by the same maker belonging to Columbia College, one of similar dimensions belonging to Lt. Ashe, a thirty-inch equatorial belonging to the Naval Academy at Annapolis, a three foot alt. and azimuth, belonging to the College of

South Carolina, and a mammoth comet-seeker, of 7½ inches aperture, by Fitz. An equatorial belonging to Mr. Rutherford of New York city, a gentleman well known for his disinterested zeal and efficient labors in the cause of astronomical advancement, served the photographers of the expedition to fix from time to time the successive phases of the eclipse. Another comet-seeker was fitted up for the purpose of furnishing an image of the sun upon a white ground in a darkened chamber.

Fifteen auroras were observed during the absence of the expedition. In nearly every instance a corona was repeatedly formed, though many of the auroral clouds were exceedingly filmy and thin.

Atmospheric electricity was almost or absolutely *nil* during the entire stay of the expedition at the observing station. The passing of the shadow seemed to produce no change in this respect.

The diurnal magnetic variation was very large, varying from two to five degrees. During the eclipse the needle was more quiet than before or after. The surface temperature of the sea was very low from the Straits of Belle Isle northward. It was frequently down to 38° or 39°. But the lowest surface temperature at any time observed, was in the Straits themselves, on the return passage; when the thermometer marked, in the surface water, 32°.

In the harbor at the observing station, ice formed in the shoals near the beach on the 13th of July. On Sunday, July 22, there occurred a storm of snow and sleet, which covered the deck of the steamer, and wrapped the whole surrounding country so far as visible, in a mantle of white. On the return passage, this snow was observed still enveloping the mountains far down the coast.

Scarcely any quarter of the world presents, perhaps, more difficult or dangerous navigation than the coast of Labrador. The islands, islets, submerged rocks and reefs are absolutely innumerable, and icebergs swarm where these more fixed dangers are wanting. The month of July was pronounced by the hardy mariners engaged in the Labrador fisheries, to have been one of the most tempestuous ever known in those seas, and most prolific of disaster to their fishing vessels. It will therefore occasion no surprise to state that the Bibb has been repeatedly in positions of hazard, requiring all the resources of her officers to meet successfully. The members of the corps cannot but feel that the hand of a protecting Providence has been more than once distinctly visible in preserving them amid dangers, and delivering them from situations, to which they can hardly look back with tranquillity. F. A. P. B.

2. *Extract of a letter from the Superintendent of the Coast Survey to the Editors in relation to observations made on the Western coast of the United States, for the Coast Survey,* by Lieut. J. M. Gilliss, U. S. N.—Lieut. Gilliss arrived at the station selected by him for observing the eclipse, and which is near Steilacoom, Washington Territory, on the 9th of July. Here he encamped and made his preparations for observations of time, latitude, etc. These are not yet definitely worked up and hence I do not give the position of the station or the times of the different phenomena in detail at present. The following particulars from Lieut. Gilliss's report will be found of interest in anticipation of the time and longitude results:

"For the first time after our arrival at the station, the sun rose clear on the morning of the 17th, nor was there at any time during that day more than two-tenths of the sky obscured by clouds. Yet, although the evening was absolutely cloudless, and the stars were shining with remarkable lustre after midnight, so fickle had been the climate during the preceding three weeks, that when we closed the tent, three hours before the eclipse would begin, I had no confidence that the next morning would be favorable for observation.

By $3\frac{1}{2}$ A. M. we were up and had removed the meteorological instruments from camp to the knoll. At that time it was sufficiently light to write without artificial aid. Mt. Rainier was distinctly visible and sharply cut against the southeastern sky. Beyond it and towards the point at which the sun would rise, there was a stratum of vapor whose upper line was slightly inclined from mid height of Rainier towards the northern horizon. At that time the barometer stood at 29·698; att. thermometer 44°·5, the temp. of the air 45°·2, and there was only a very slight air from the southward. At (0^h 17^m, sid. chron.) the mist striæ became dense to the N. and E., and were more evidently in rays diverging from the point of sunrise to an elevation of some 25°. The air was so cool and so loaded with moisture that although the telescope had been out all night the object glasses were densely covered with dew immediately after the caps were removed. By (0^h 30^m) a part of the vapor in the N. and E. had condensed into little cumuli beyond the Cascade range each more light and feathery with distance from diverging point, though none of this series extended as far as Mt. Rainier, and it was only towards the north that a dense volume of vapor could be seen coming in towards the lower lands bordering on Puget's Sound. Two minutes later and the edges of the little flocculi were tipped with pink and golden hues increasing in brilliancy of color as the sun approached the horizon.

The eclipse had far advanced when the first cusp appeared above the horizon at (0^h 39^m 38^s). It was seen through a red screen glass and was sharp and without tremor. Indeed the atmosphere was so still that the rise of the second cusp over the distant ground line at (0^h 40^m 58^s) was observed almost with the precision of a transit of a limb over the wires of a telescope. But it was at once perceived that there was great disturbance of the lune, the lower half being flattened by the unequal refraction.

At this time I was again obliged to wipe the heavy drops of dew from the object glass of the telescope, and whilst so doing my attention was directed to the vapor near us. The whole northeastern portion of the prairie had apparently been converted into a placid lake with here and there a knoll projecting through and forming a minature isle, the illusion being enhanced by rapidly diminishing intensity of the light. At (0^h 54^m) distant objects could not be recognized more distinctly than during midsummer twilight at $8\frac{1}{2}$ P. M.

At (0^h 55^m) the southern cusp had become rounded off and rugged as though the moon's edge was serrated. But had such been the case this portion of the lune would have broken into beads of light before the total obscuration took place, and that did not occur, the moon's disc equally and uniformly interposing between us and the sun until the last glimmer of light disappeared.

I had turned off the red screen half a minute before and was surprised to behold quite distinctly the following segment of the lunar sphere. The periphery of this segment was more than 100°. Its color was uniformly shaded from an intense black at the centre of the lunar disc to a very dark grayish purple near the limb of the sun. It was still traceable during twenty seconds after the last glimmer of sunlight.

At the moment of totality beads of golden and ruby-colored light flashed almost entirely around the moon not constant even for a second at one point but fitfully flashing as reflection from rippled water, and as mutable in the respective places of the colors. This bead-thread could not have extended more than ten seconds beyond the lunar disc. It broke up suddenly at ($1^h\ 16^m\ 21^s.2$ sid. chron.) and then for the first time protuberances were noted beyond the following limb of the moon. The position of its largest one was 75° or 78° W., and in the form of a flattened cone or pyramid of cumulus cloud about one minute in height and when first observed perhaps two minutes broad at base. The cloud was not a uniform mass but apparently an aggregation of small ones, and its general tint was a rosy pink with occasional spots and edges of yellowish white light as though sunlight shone obliquely through them. Except in the pink color it greatly resembled the protuberance noted during the total eclipse of the sun observed at Olmos, Sept. 7th, 1858. As the moon advanced this protuberance was probably broader at the base and brighter at the summit while its apparent elevation remained the same. This was an extremely beautiful sight, and I watched it closely, giving nearly all my attention to it during 15^s, yet at the same time I was able to perceive a lesser one of a more flattened appearance distant 10° or 15° towards the west, and several others yet smaller and one long one of a much darker color at different points of the disc. Intently occupied with the great protuberance, the corona had not been recognized up to this instant ($0^h\ 58^m\ 10^s$), interest in the former causing me also to drop the beat of the chronometer.

It was then so dark that I found it impossible to recognize the second's dial of the chronometer (the gold one) and Mr. James Gilliss was called to bring his lantern and read the time at which I should indicate the second internal contact of the limbs. Raising my face from the box on which the time keeper stood to the telescope a most extraordinary scene was apparent! Over the moon's black disc colors of the spectrum flashed in intersecting circles of equal diameter with that body, and each apparently revolving towards the lunar centre. The moving colors were not visible beyond the moon, but a halo of virgin white light encircled it, which was quite uniformly traceable more than a semi-diameter beyond the black outline. This corona was composed of radial beams or streamers, having slightly darker or fainter interstices rather than a disc of regularly diminishing or suffusing light; but the gorgeous appearance of the spectrum circles with their incessantly changing bands of crimson, violet, yellow, and *green*, thoroughly startled me from the equanimity with which the preceding phenomena had been observed. Nor were these colors physiological results from a change of position of the body, or of preceding strain of sight in efforts to recognize the division of the second's dial, in darkness, and subsequent direction of the eye towards the

sunlight, for they continued visible with the telescope at least 10^s longer. As near as it was posible to estimate, the breadth of each spectrum circle was about two minutes. The green colors were not darker than the tint usually called pea green, and were on the edges farthest from their respective centres, but neither of the lines seemed to retain a definite position, and I was irresistibly drawn to their contemplation to the neglect of all the changes that might have been taking place in the protuberance and corona.

They vanished with the first appearance of sunlight beyond the western limb of the moon, their sudden obliteration causing me to utter an exclamation which was regarded as the signal for noting the time, a datum whose importance had been wholly forgotten in the fascination thus caused. I cannot liken them to anything so nearly as to the image seen in the kaleidoscope."*

3. *Observations made during the Total Eclipse of 18th July, 1860, on the summit of Mount Saint-Michel, in the Desert of Palmas, Spain;* by A. Secchi, S. J. Communicated to the French Academy of Sciences. (*Comptes Rend.*, li, p. 156, July 30, 1860.)—The place where I observed the eclipse was on the top of Mount Saint-Michel, in the desert of Palmas, at the same point chosen by Biot and Arago for their triangulation operations, and at a height of 725 metres (=2378 ft.) above the sea, commanding a very wide horizon. The weather was magnificent during the whole time of obscuration, notwithstanding we were tormented with a cruel anxiety up to a few moments of the time by parasitic clouds forming continually on the mountain and dissolving only when at some distance. But they happily disappeared just before the critical moment, and the sky was fine till evening.

I was accompanied by M. de Aguilar, director of the Madrid Observatory, and by Mr. Cepeda, lawyer from Valencia, a distinguished amateur.

The instruments were a Fraunhöfer telescope of 78 mm. aperture (about 3·04 inches) and 1^m·20 (=47¼ in.) focal length, with powers of 60, 90, and 130 diameters. The two first powers gave the entire disc of the sun, and the three oculars being mounted on a slide could be changed with the greatest rapidity. The micrometer carried a system of six spider lines, with spaces of 6′ (invisible in the dark), and of four very fine platinum wires so disposed that there was a space just equal to one lunar diameter between the outer wires—the two intermediate wires were slightly inclined, subtending 1′ 30″ at the narrower, and 2′ 30″ at the larger angle; an arrangement designed to aid in obtaining a more exact estimate of the protuberances. The whole micrometer revolved on a transom, with a plate on which was a graduated circle and a sheet of white drawing paper upon which the angular position was marked by touching a pencil, carried by the fixed transom—thus reserving the reading until the observations were concluded. This instrument was mounted equatorially, was very stable, and had been adjusted the day before.

* Up to this date, Sept. 7th, the party sent to the Cumberland House, British Columbia, have not been heard from.—Eds.

Some minutes before the commencement of observations I verified the position of the telescope and the commencement was marked by a Morse telegraph, kindly procured from Madrid by M. Aguilar, provided with a pendulum which marked the seconds. A simple mechanism marked the instant of observation. Some minutes after the commencement I sought for the disc of the moon outside of the sun but could not make it out. At $2^h\ 19^m$ I succeeded in seeing very clearly through an arc of about 10° or more, but some time after the moon disappeared, and after that it could be observed only for an instant. Is this due to the inequality of the portions of the solar corona, upon which the disc of the moon is projected? I observed with certainty that not only the edge of the solar crescent was more sharply defined upon the side of the concave phase, than upon its own proper border, but also that the field of the telescope was much more clear upon this side than upon that of the moon, and the same could be seen in projecting the solar image on white paper.

The cusps remained throughout very distinct, and the solar spots were successively eclipsed without any distortion as viewed with a magnifying power of 90. The lunar mountains were well outlined upon the solar ground, and indented the inner border of the limb. After the centre of the sun was hidden (and even a little more) the light of the horizon diminished suddenly in a decided and unexpected manner. Surrounding objects did not however noticably change color. As the eclipse was becoming nearly total, I took away all the fixed colored glasses, and followed the sun with a glass held in the hand. This was an excellent glass of neutral tint made by Lerebours, a graduated light, the lighter shade being very delicate. The slender crescent is now breaking into many parts near the cusps which still remain very distinct, and the corona begins to show itself even with the dark glass. The sun reduced to a simple thread disappeared without forming (*grains des chapelet*) Baily's *Beads*. Quickly taking away the colored glass I was surprised to see the sun yet white and its light so strong that it hurt my eyes, but its splendor visibly diminishing and changing to a purple light, which at once appeared to terminate in an infinity of purple points, which were as soon hidden, and then two great red protuberances appeared near the point of occultation. One of these was at least $2'\ 30''$ in height, and as large at the base as $2'$; it was conical in form, slightly tapering, curved at the top. The other was about half the height of the preceding, extending over a considerable arc, at least 5° upon the solar border. Its top was formed like very minute saw teeth, parallel to the border of the moon. I looked as soon as possible at the opposed margin of the sun, but nothing appeared. Returning to the first margin I saw that the protuberances were rapidly hid. During the whole time the corona was magnificent but most brilliant on the side on which the eclipse began. Its light was all around uniform and without interruption, of a beautiful silver white, shading off gradually from the margin of the moon to the distance of about the lunar radius or less. At this distance there began to be many interruptions; large sheafs of light appeared, those of the upper part were the largest, and extended out to a distance equal to a diameter and a quarter of the moon. On

the lower part I saw only one of these long sheafs. With Arago's polariscope, already directed very near the sun, I ascertained that the two images were not of equal brilliancy, and that the corona in one was lengthened in one direction, and in the other in a direction perpendicular to the first, but I could only give some seconds to their examination.

Returning to the telescope, I regarded for an instant the imposing scene which was then displayed in all its majesty. The moon perfectly black showed itself with all the glory of its rays, which appeared lengthened below and to extend out to the distance of two solar diameters. The heavens were of a light ash color, but not of threatening aspect. Near objects were plunged into feeble twilight contrasting with distant objects not yet in shadow. All this unique scene remains profoundly engraven on my mind; the solemnity of the spectacle appeared forcibly to impress the assistants, who, though numerous, all remained in perfect silence. Not to lose these precious moments I returned immediately to the telescope. The aspect of the sun was much changed. The two great protuberances of which I have spoken had already disappeared, and a great number of others appeared from on all sides of the sun (this moment corresponding to the middle of the total obscuration), I was for an instant embarrassed to decide which to select for measurement of their angle of position; for it was useless to measure the size, which changed while looking at it. With the mechanism of the micrometer in a few seconds I determined six—although I counted at least ten—there was hardly any part of the surface of the disc where there was not a point, they seemed regularly distributed. These are the angles taken in reckoning from east to northwest. South: 39·0°, 75·0°, 116·0°, 173·0°, 211·3°, 310·0°.

A greater brilliancy of the corona on one side announced that the sun was emerging, then in directing my attention to this side I was astonished to see a very large number of very small protuberances, and above all of them a red cloud entirely detached which was suspended and separate from the rest and from the lunar margin by a marked white space. Its figure was elongated, about 30″ of length to 3″ of width, and its form somewhat tortuous and sharp at the extremities (I called to my companions to witness this). This cloud was not alone. I had the conviction that it was accompanied with many others which rested at nearly the same level as a series of cirrus. Their color was that of the protuberances, only a little more distinct.

During all this the number of protuberances increased greatly upon this side and soon assumed a continuous arc formed like a saw, which extended at least to 60° of the circumference, and which gradually lengthened while its central part increased in width and brilliancy. The purple color mingled with the white in gradual transition until the white became so strong that the eye could not sustain it; the protuberances then disappeared.

The sun then began to shine in the heavens like a point of true electric light, and made a singular contrast with the corona still remaining, and which (by hiding with my hand the bright part) I could see for 40 seconds longer. That which most struck me in the circumstances, was the immense quantity of red protuberances and their distribution; so

that one could absolutely say they enveloped the sun. Those commonly observed are only the summit of the most elevated, and without doubt it is only in certain favorable circumstances that we can see the sun entirely crowned by them. This corona of light prevented accurate observations of time, and it would give a different solar diameter according to the depth of shade of glass employed.

The time of total obscuration was found by M. Cayetano de Aguilar to be $3^m\ 11^s$, but it passed like a moment and seemed to us, at the most, not more than two minutes.

My convictions upon the nature of that which I saw are that the phenomena were real and that I truly saw the flames in the solar atmosphere and clouds suspended in these flames; it would be impossible to imagine anything else, as for example, that it might be some phenomena of diffraction or refraction.

The clear graduation and distinct mingling of the peach blossom colored light with the white photosphere was of a character so distinct that it can never be mistaken by any phenomena of interference, of refraction, or any illusion whatever. I do not doubt that it really appertains to the sun, and the structure of these suspended clouds tends to strengthen my conviction. As regards the part of the corona more remote and those long bundles of rays, the thing does not appear to me so certain: they have too much the aspect of those seen through the clouds at sunset. Yet it is important to distinguish from these the true corona which was continued much beyond the protuberances.

M. Cepeda, however, who made his observations with an excellent telescope, having a large field, assures me that he saw a bundle of rays, curved and branched like the horns of a stag, at the upper part.

All these observations have been confirmed by photography.

The director, M. Antonio de Aguilar, had brought, for making the solar photographs, the large telescope of Cauchoix, mounted upon a solid cast iron foot, furnished with clock-work. Besides numerous proofs of the entire sun, he took 14 impressions on a larger scale, and 5 of the natural size of the focal image, 23 millimeters in diameter, and which represent all the phases of the phenomena. The examination of these photographs will be made under more favorable circumstances with proper instruments. At present, I will only say that the times of exposure varied from from 3^s to 30^s, that all the images are solarized in the protuberances, but the corona has an intensity differing according to the time. There was not the same intensity throughout but the most vivid parts do not correspond to the protuberances.

We notice also a greater intensity in the chain of protuberances toward the first and the last instant of total occultation. The force of the light of the protuberances is such that one impression is become triple by a momentary jar of the telescope. In this delicate operation M. Monserat, Professor of Chemistry in the University of Valentia, was charged with all photographic operations, and my compeer, P. Vinader, took charge of the regulation of the telescope. This communication has already become so long that I omit ordinary observations, and will only say that the light was strong enough to enable one to distinguish small objects, and to read without difficulty ordinary books, and without

seeking I saw Jupiter, Venus, and some other luminaries. A portion of this light may have proceeded from the reflection of a thunder cloud, a short distance from and feebly lighted by the sun.

I give the result of the observations made with the thermomultiplier of Melloni by M. Botella, inspector of mines. In general the progress was very regular, as the figures show:

Commencement,	1h.	57m.	Galvanometer,	20·0
	2	11	"	18·3
	2	25	"	15·5
	2	35	"	11·5
	2	58	"	2·0
	3	5	"	1·5
Totality,	3	10	"	0·0
Emergence of the sun,	3	12	"	0·5
	3	20	"	1·0
	3	35	"	12·0
	3	55	"	15·0
	4	16	"	17·5
End,	4	30	"	20·0

A very sensitive declinometer of Jones, observed hourly by M. Mayo, engineer, showed no disturbance. Professor Barreda observed the solar spectrum at my request, and will give his report thereon in a special memoir.

4. *On the polarization of the light of the corona, and of the red protuberances, in total solar eclipses.*—M. Prazmowski observed at Briviesca in Spain, with special reference to these subjects, the total eclipse of July 18th. His observations seem to justify the following conclusions, viz.

(1.) The light of the red protuberances is not polarized. In this respect they resemble clouds in our atmosphere. May we hence conclude that these are solar clouds, composed of particles, not gaseous, but liquid or even solid? The high temperature of the sun leads us to infer that these clouds are constituted of very refractory matter.

(2.) The polarization of the corona proves that its light emanated from the sun and was reflected. The bright, very decided, polarization, proves also that the gaseous particles from which it was reflected send the light to us reflected nearly at the maximum angle of polarization. For a gas this angle is 45°; but in order to reflect light at this angle it must be near the sun. A solar atmosphere seems to furnish the necessary conditions.—*Comptes Rendus, Août* 6, 1860.

5. *Baily's Beads.*—Mr. Lespiault, who watched especially for this phenomenon, says—(Compt. Rend., li, 221)—some seconds before the first interior contact, the margin formed by the arc of the moon appeared irregular and trembling, but I did not see either the "*Baily-Beads*" or "*comb-teeth.*"

6. *Third Comet of* 1860.—A brilliant comet, with a tail several degrees long, was seen by many persons in different parts of our country, on the evening of June 21st and 22d, 1860. It was seen on the evening of June 20th, by Prof. Caswell of Providence, then on the deck of the steamship Arabia. The first public notice of the comet appears to have been made by Mr. C. W. Tuttle, assistant in the Harvard College Observatory. The comet continued vissible to the naked eye about two weeks.

The following parabolic elements of its orbit were computed by Mr. Tuttle, from observations at Cambridge, Mass., of 21st, 24th and 27th of June.

Perihelion passage,	1860, June 16.06730.	
Long. of perihelion, - - -	161° 34′ 56″	Mean eqx.
" " asc. node, - - -	84 41 20	Jan. 0.
Inclination, - - - -	79 18 11	
Log. of perihelion distance, -	9.46687	
Motion, - - - - - -	*direct.*	

Gould's Astron. Jour., No. 136.

7. *The Meteor of July 20th*, 1860.—This remarkable meteor was visible over a portion of the earth's surface at least a thousand miles in length (from N. N. W. to S. S. E.) by seven or eight hundred in width; or from Lake Michigan to the Gulf Stream and from Maine to Virginia. The newspapers have contained many notices of its appearance as seen at various places within these limits, but most of these accounts are too vague to be of any scientific value. We are not yet in possession of a sufficient number of good observations for a final discussion of the phenomena presented, and can only at this time notice briefly a few of the best that have come to hand, and state some approximate results derived from them respecting the height of the meteor above the earth, the direction of its path, &c.

At New Haven, it was seen, during a portion of its flight, by several members of the Scientific Faculty of the College at the house of Prof. J. A. Porter, and pains were at once taken to fix its apparent path by reference to parts of the building, tree-tops, stars, &c., near which it had been seen, and also to determine its time of flight, by noting the time required to repeat the various acts performed while it was in sight. The bearings and altitudes of the points noted for fixing the path were subsequently determined instrumentally. Independent data of the same kind were also obtained by going with many different observers to the places occupied by them at the time, and observing with compass and quadrant the path in the sky pointed out by each, and noting the time for each in the manner already indicated.

By laying down these bearings and altitudes on a globe, a normal or average path was obtained, which cuts the horizon at N. 62° W. and S. 62° E., and gives a maximum altitude of 53°, in a direction S. 28° W.

The time of flight for the different observers, determined as above stated, ranged from 10 to 20 seconds—giving an average of fourteen or fifteen seconds, which agrees with the careful estimate made at the time by the observers at Prof. Porter's.

Valuable observations have also been received from individuals in different places, some items of which we proceed to state. They will be given more fully hereafter.

Mr. J. D. Lawson, of New York, saw the meteor from the corner of Fourth street and Broadway, and has furnished data which give for maximum altitude (N.) 56¾°. Another independent observation at the same spot, as published in the *Journal of Commerce*, gives from data subsequently obtained by Prof. H. A. Newton, an altitude of about 55°. We use for N. Y. 56° as the mean of the two.

Mr. F. Huidekoper, of Meadville, Pa., makes the altitude at that place 39° 30′ from the northern horizon; the point of disappearance at altitude 3° 30′, and 10° 45′ S. of east; time from crossing meridian till disappearance, 10 to 12 seconds.

Mr. W. King, a surveyor at Erie, Pa., makes the altitude 44°, and point of disappearance in a cloud due east at an altitude of 22°.

Mr. S. B. McMillan, of E. Fairfield, Ohio, reports it as having been seen "moving from a point about 10° E. of N. to within as much of a due east direction," attaining an altitude of 15°.

Rev. T. K. Beecher at Elmira, N. Y., saw it pass very nearly through his zenith, and "so very close to" α Lyræ "as to quench, if not eclipse it." This star was then about 11° from his zenith, and in azimuth S. 76½° E. The meteor separated into two parts with an explosion when near the zenith.

Other observations (not now at hand), which have been used in obtaining our results, have been received from Mr. B. V. Marsh, of Philadelphia, and Prof. Hallowell, of Alexandria.

A comparison of these observations, and a few of the best that have been published, give approximate results as follows:

(1.) The vertical plane in which the meteor moved cuts the earth's surface in a line crossing the northern part of Lake Michigan, passing through, or very near to, Goderich on Lake Huron, (C. W.), Buffalo, Elmira and Sing Sing, N. Y., Greenwich, Conn., and in the same direction across Long Island into the Atlantic.

(2.) In this plane the path that best satisfies the observations is sensibly a straight line approaching nearest to the earth (41 miles) at a point about south of Rhode Island, and having an elevation of 42 miles above Long Island Sound, of 44 over the Hudson, 51 at Elmira, 62 at Buffalo, 85 over Lake Huron, and 120 over Lake Michigan. The western observations, however, which are few and imperfect, seem to indicate a somewhat greater elevation than this for the western part of the path. Possibly, therefore its true form may have been a curve convex towards the earth, resulting from the increasing resistance of the atmosphere as the meteor descended into denser portions of it. The observations made this side of Buffalo, which are somewhat numerous and many of them good, are very well satisfied by the straight path already described. Further and more accurate observations beyond Buffalo are greatly needed for determining the true form and position of the orbit, both in respect to the earth's surface and in space.

(3.) The close approximation to parallelism to the earth's surface of the eastern portion of the observed path leaves it a matter of doubt, considering the imperfection of the observations, whether the meteor finally passed out of the atmosphere and went on its way in a disturbed orbit, or descended gradually into the Atlantic. The former supposition is perhaps the more probable, especially if the path was curved, as above suggested, instead of a straight line.

(4.) The meteor exhibited different appearances in different parts of its course. It seems to have been observed first as a single body, more or less elongated, gradually increasing in brilliancy, throwing off occasionally sparks and flakes of light, until it reached the neighborhood of El-

mira, N. Y. Here something like an explosion occurred, and the meteor separated into two principal portions with many subordinate fragments all continuing on their course in a line behind each other, and still scattering luminous sparks along their track, until a point was reached about south of Nantucket, when a second considerable explosion took place, and afterwards the principal fragments passed on till lost to view in the distance. The most trustworthy observations represent the meteor as disappearing while yet several degrees above the horizon, (generally from 3° to 6° or 8°). Besides the actual changes of form which the body successively underwent, apparent changes would present themselves to each observer arising from change of direction in which the meteor was seen.

(5.) It is not easy, from the observations in hand, to determine with much accuracy the velocity of the meteor while passing through our atmosphere. The time of flight is doubtless largely overestimated by most observers, especially those unaccustomed to measure intervals of a few seconds. Timing with a watch a repetition of the acts performed during the flight of the meteor, usually reduces the interval to not more than one third, or even one fifth, of the observer's own estimate. From 15 to 30 seconds is a fair range for good observations, and probably to no observer was the meteor in sight over 45 seconds or a minute, although a minute and a half and two minutes are very common estimates. A comparison of the most probable estimates of time with the length of path observed, gives a velocity ranging from eight to fifteen miles a second. Probably 12 or 13 miles is a tolerable approximation. This, allowing for the earth's motion in its orbit, gives 26 or 27 miles a second as the actual velocity of the meteor in space. Its relative velocity may have been much greater when just entering the atmosphere, than after encountering its accumulated resistance.

(6.) The actual diameter of the luminous mass, taking its apparent diameter as nearly equal to that of the moon, (the estimate of many observers nearest its track) must have been from one-fifth to one-third of a mile. Many estimates would make it still larger. The two principal heads when passing New Haven must have been from one to three miles apart.

(7.) A report is mentioned by many observers as having been heard from one and a half to five minutes after the meteor passed. The least time in which such a report could have been heard, taking the usual constant for the velocity of sound (1090·47 feet a second) would be about three minutes and a half. This is a point of much interest, and needs to be investigated.

The "rushing sound" spoken of by many as heard while the meteor was passing, is of course to be attributed to imagination.

C. S. Lyman.

8. *The Meteors of August 2d and 6th*, 1860.—A meteor, rivaling in brilliancy that of July 20th, was extensively observed throughout the Southern United States on the evening of August 2d, between 10 and 11 o'clock, according to the local time. It appears to have passed from east to west vertically over Tennessee at ten minutes past ten, Knoxville time. "From three to five minutes after the disappearance of the meteor a report was heard like the discharge of an eight-pounder; which was fol-

lowed by a long—*long* rolling, reverberatory sound of more than a minute's duration." This fact is mentioned by Mr. W. C. Kane writing from Knoxville to a friend in Hartford, Conn.

Another brilliant meteor was seen in the southwest, from New Haven and New York, between half past seven and eight o'clock, on the evening of August 6th. It passed from south to north, and notwithstanding the daylight still remaining attracted attention over a wide extent of country.

9. *Further Notice of the New Concord (Ohio) Meteor, of May* 1, 1860; by Prof. E. W. Evans.—Since writing my communication published in the July number of this Journal, on the path and height of the New Concord meteor, I have found some additional data, which I regard as important because they have been furnished by a good observer who saw the meteor under favorable circumstances. A single case of this kind is the more worthy of note because, owing to the cloudiness of the day when this meteor passed, there were but a few places from which it was seen at all. The observer referred to is D. Mackley, Esq., a lawyer of Jackson, Ohio, who at the time of the occurrence happened to be at Berlin, about six miles north east from the former place, and seventy miles from the nearest point under the meteor's path. He took pains to note all the facts as accurately as he could at the time; and he afterwards returned to the spot in order to determine more definitely the points of the compass. His testimony, in answer to my interrogatories, is substantially as follows:—

"The meteor first appeared to me at a point about 55° east of north. It moved northward in a line very nearly parallel with the horizon. When it disappeared it had described an arc of about 15°. It was in sight about 6 seconds. Its altitude was about 30°. In regard to its size, I have since looked at the sun through a thin cloud, and I think the apparent diameter of the meteor was one-half that of the sun."

These data give the meteor a height of 41 miles over the northern boundary of Noble county; a diameter of three-eighths of a mile; and a relative velocity of nearly 4 miles a second. The results agree sufficiently well with those before given.

The meteor was seen through openings in the clouds at various points along a line of 60 miles, extending from near Newport on the Ohio river to the neighborhood of New Concord. The evidence, upon the whole, does not indicate any descent of the body towards the earth between these limits, or any change in its size or appearance. From this fact, and the great height of the body, and the absence of all evidence that it was seen or heard in the northern part of the State or beyond, it seems probable that this meteor was not dissipated in the atmosphere, but passed out of it again. The shower of stones which came down near New Concord had probably been detached from the principal mass before the latter came into sight.

Marietta, Ohio, Aug. 20, 1860.

10. *Shooting Stars of August* 9–10, 1860.—Since the year 1837, at least, it has been found in the Northern hemisphere, whenever the weather has permitted observation, that shooting stars have been unusually abundant during a period of several nights in August, gradually increasing in numbers for a few days up to the 10th of the month,

and then gradually diminishing in frequency. While every other meteoric period has intermitted, this of August holds out with little change.

The observations below stated were made in the open air, from the top of the southern tower of the Alumni Building of Yale College, by a corps of observers consisting of J. W. Gibbs, Jr., Charles Tomlinson, J. W. Johnson, W. C. Johnston, H. W. Siglar, J. B. Chase, with myself. During the hour from 2^h to 3^h A. M., there were but six observers. Each meteor was called aloud, and no one was reckoned twice. Between 10 P. M. of the 9th and 3 A. M. of the 10th of August, 1860, we observed *five hundred and sixty-five* different shooting stars, distributed as follows, viz:

Aug. 9.	10^h to 11^h P. M.	N. E.	29	11^h to 12^h P. M.	N. E.	14
		S. E.	30		S. E.	22
		S. W.	35		S. W.	47
		N. W.	11		N. W.	18
			105			101
Aug. 10.	0^h to 1^h A. M.	N. E.	19	1^h to 2^h A. M.	N. E.	24
		S. E.	27		S. E.	25
		S. W.	44		S. W.	31
		N. W.	17		N. W.	17
			107			97
	2^h to 3^h A. M.	N. E.	30			
		S. E.	20			
		S. W.	65			
		N. W.	40			
			155			

The meteors were reckoned in that quarter where they first became visible, but it is of course impossible to indicate satisfactorily by words their distribution on the face of the sky. The majority of them were inferior in brilliancy to stars of the second magnitude. Many however were as bright as stars of the first magnitude, and a few rivalled in splendor Mars and Venus. Several of them left luminous trains, but none appeared to explode. Their general Southwesterly direction was evident, and as nearly as we could estimate, at least three fourths of all conformed to the radiant of former years, in the vicinity of the sword-handle of Perseus.

During our watch the sky was clear except that between 1^h and 2^h A. M. there were some clouds about the west. The moon in her last quarter embarrassed our view after 11 P. M., and doubtless concealed about one third of all the meteors which might have been seen in her absence. At 3 A. M. mist was forming rapidly, but we saw after that hour *five* meteors not included above; and while we were arranging for the watch a few minutes previous to 10^h we saw *eighteen*, making a total of 588 meteors observed by us during five hours and ten minutes. Had the moon been absent we should probably have seen 800 during this period. This is not far from six times the common nightly average for a like interval.

It is perhaps hardly worth mentioning that the Aurora Borealis was visible all night, being by turns quite active after twelve o'clock. This phenomenon has been seen here with uncommon frequency this summer, but it appears to have no special connection with shooting stars.

During the entire nights of the 8th, 10th, 11th, 12th and 13th of this month, our sky was wholly overcast. EDW'D. C. HERRICK.

Yale College, New Haven, August, 1860.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The Fourteenth Meeting of the American Association for the Advancement of Science*, was held, August 1st–8th, 1860, at Newport, R. Island, in the old State House, dating from the Colonial times under Queen Anne. The Hon. W. H. Cranston, Mayor of Newport, pronounced an address of welcome from the city of Newport, to the members of the Association. The general enjoyment of the occasion was amply secured by the kind and abundant hospitality of the city and its citizens, leaving on all who participated the most agreeable impressions of Newport. The weather was superb, neither fog or rain marred the pleasure of the unequalled drives by beach and shore over this American Isle of Wight, and the Geologists with Mr. Hitchcock's map* of the Island in hand found the conglomerates of 'Purgatory' and 'Paradise' and the graphitic anthracite of these most overdone and contorted coal measures a fruitful theme of discussion both in hall and field.

The numbers in attendance at the Newport meeting were small compared with most previous meetings. An erroneous impression appears unfortunately to have gone abroad that Newport was a very costly place to visit and that the 'height of the season' at a great watering place was not a time when science could hold ground against fashion. While all this was eminently untrue, the effect of such an impression was visible in the absence of numbers of familiar faces.

Neither can we, if we would, conceal the fact that while many papers of marked ability were presented, the character of this meeting was not in all respects creditable to American Science. A conviction prevailed among many who were present at Newport of a decadence in the scientific character of the Association, of a loss of tone which if not already a demoralization threatened soon to become such. There was a time when weak speculations—by whomever put forth—were promptly trampled on and their authors made to feel that they were answerable to a power of united public sentiment which bore rule with a righteous severity. The evidence of this power was wanting at Newport—signs of concession to scientific charlatanism were visible in quarters where we least looked for it, and we left the meeting with a persuasion that unless the old order of proceedings could be restored, the American Association must come to a speedy and disastrous end. We do not despair of the immediate restoration of a higher standard—the effort will certainly be made.

One of the bright points of the Newport meeting was the opportune return of the Labrador Astronomical Expedition, sent out under the auspices of the United States Coast Survey and under the immediate sci-

* Furnished gratuitously to all members by the City Government.

entific leadership of Prof. Stephen Alexander of Princeton, to witness the total Eclipse of July 18th. As we elsewhere give a detailed statement of the results from the pen of one of the expedition, it is only necessary to add that the "*Bibb*" made Newport harbor on her return in season to secure an agreeable reunion on Tuesday evening, and that on Wednesday morning Prof. Alexander recounted the results of the Expedition to that land of 'monumental desolation' in a brief but fervid recital, deeply moving his audience by his peculiar eloquence. It was a happy circumstance that the meeting could close under so agreeable an impression.

The address of the retiring President, Prof. Stephen Alexander, was delivered on Wednesday morning, just before the adjournment, and will appear in the Transactions of the year.

Prof. Bache gave an interesting public discourse reviewing the history of discovery, and the method of investigation employed in regard to the *Gulf Stream*. This address we hope to present to our readers in full in a future number of this Journal, as also the substance of another public address by Prof. Henry, on Atmospheric Electricity, delivered as a popular evening lecture at Newport, by the distinguished Secretary of the Smithsonian Institution.

List of papers presented to the Association.

Section A—Mathematics, Physics and Chemistry.

General Account of the Results of Part II of the Discussion of the Declinometer Observations made at the Girard College, Philadelphia, between the years 1840 and 1845, with special reference to the Solar Diurnal Variation and its Annual Inequality; by A. D. Bache.

On some of the Relations of the Violet and Green Modifications of Chrome Alum to Soda, Potassa and Ammonia; by E. N. Horsford.

Reflections on the Origin, Development and Changes of Languages; by Henry M. Harman.

The Great Auroral Display of August 28 and September 2, 1859; by Elias Loomis.

Brief Abstract of a Memoir on the Theoretical Determination of the Dimensions of Donati's Comet; by W. A. Norton.

On the Solution of Ice in Inland Waters; by B. F. Harrison.

On Natural Ice-Houses and on Frozen Wells; by Elias Loomis.

Description of a new Registering Thermometer; by James Lewis, of Mohawk, N. Y. Presented by W. B. Rogers.

On the Phenomena presented by the "Silver Spring," in Marion County, Florida; by John LeConte.

Influence of difference in the mean velocity of winds from the different points of the compass, in modifying the mean direction of the atmospheric currents over the United States; by James H. Claflin.

Can the Sudden Cooling of one Part of a Metallic Rod cause another Part, as a consequence, suddenly to become warm? by E. N. Horsford.

Improvement in Barometers; by H. A. Clum.

General Account of the Results of Part III of the Discussion of the Declinometer Observations made at Girard College, Philadelphia, between the years 1840 and 1845, with special reference to the Investigation of the Influence of the Moon on the Magnetic Declination; by A. D. Bache.

Some experiments and inferences in regard to Binocular Vision; by W. B. Rogers.

On the data and methods of the Hindu Astronomy; by W. D. Whitney.

On Atmospherical Electricity; by Joseph Henry.

Note on Sources of Error in the employment of Picric Acid to detect the Presence of Potash; by M. C. Lea. Presented by B. Silliman, Jr.

On a series of new combinations of Ammonia Picric Acid and Metallic Bases; by M. C. Lea. Presented by B. Silliman, Jr.

On the Combustion of Wet Fuel; by B. Silliman, Jr.

Theory connected with the Solar Spots; by C. W. Hackley.

Description of a new Portable Coffer Dam for Foundations under water; by E. B. Hunt.

On certain Variable Stars; by B. A. Gould.

Modern Warfare; its science and art; by E. B. Hunt.

Abstract of the principal results of the observations of the Tides at Van Rensselaer harbor, made by the second Grinnell Expedition under command of Dr. E. K. Kane, U. S. Navy, during 1853, '54, '55, from a reduction and discussion by Charles A. Schott, Assistant Coast Survey. Presented by A. D. Bache.

On Catachroism, a new optical property belonging to certain Crystalline Surfaces, with some remarks on Polarization, by reflection from colored surfaces; by M. C. Lea. Presented by B. Silliman, Jr.

On a new theory of Light, proposed by J. Smith, of Manchester, England; by O. N. Rood.

Some reflections on the Observations of the Solar Spots and of the Magnetic Variations; by James Hyatt.

On the Actinism of the electric discharge in vacuum tubes; by W. B. Rogers.

Investigation of the problem regarding the existence of a lunar tidal wave on the great fresh-water lakes of North America; by J. D. Graham.

On the Hydrates of Sesquioxyd of Chromium; by E. N. Horsford.

On a series of investigations, on the assimilation of gaseous nitrogen of plants, conducted during the year 1857, 1858 and 1859, at Rothamsted, England; by E. Pugh.

Vital Statistics of the Blind, with an approximate Life Table; by E. B. Elliott.

Influence of difference in the mean velocity of winds from the different points of the compass, in modifying the mean direction of the atmospheric currents over the United States—discussed by the direction and at the expense of the Smithsonian Institution, from materials collected in its archives for the years 1854, 1855, 1856 and 1857; by James H. Coffin.

An attempt to estimate the Height, Velocity, &c., of the Meteors of the evening of July 20, 1860; by H. M. Harman.

On the meteors of 1859, Aug. 11, and 1860, July 20; by B. A. Gould.

On the Induction-Time in Electro Magnets; by A. D. Bache and J. E. Hilgard.

The loss by fire of Berdan's Mechanical Bakery in Boston, due to Spontaneous Combustion; by E. N. Horsford.

Observations on Hydraulic Cements; by Q. A. Gilmore. Presented by E. B. Hunt.

On the Motions of Uranus; by Truman Henry Safford.

Fire Damp Explosions in Collieries by lighting with coal gas; by E. B. Hunt.

The Meteor of July 20th, as seen from Washington, Dutchess County, New York; by James Hyatt.

Some remarks on the Open Sea of the Arctic Regions; by W. W. Wheildon.

On the Paradoxes of the Atomic Theory of Chemistry, with a proposition of a new Hypothesis; by Clinton Roosevelt.

On the production of Ethylamine by reaction of the Oxy-Ethers; by M. C. Lea. Presented by B. Silliman, Jr.

Abstract of the principal results of the astronomical observations at Van Rensselaer harbor and other places near the northwest coast of Greenland, made by the second Grinnell Expedition, under command of Dr. E. K. Kane, U. S. Navy, during 1853, '54, '55, from a reduction and discussion by Charles A. Schott, Assistant Coast Survey. Presented by A. D. Bache.

A new Ammonia Chrome Alum, and the Violet, Green and Red Modifications of Chrome Salt; by E. N. Horsford.

On the Time Necessary to Double the Pressure of Steam under certain circumstances; and the practical lessons therefrom; by James Hyatt.

Systematic Views on Mechanics and Mechanism, with inferences on Organisms; by E. B. Hunt.

On the nitric acid and ammonia in rain water, collected Sept., 1859, between New York and Liverpool about the middle line of the Atlantic Ocean; by E. Pugh.

On the Relations of Salts of Zinc and Alumina to Soda and Potassa; by E. N. Horsford.

On our inability through the retinal impression alone, to determine which retina is impressed; by W. B. Rogers.

The Theory of Probabilities applied to determine the Identity of Words and Languages; by H. M. Harman.

On the possibility of expressing the polar co-ordinates of the Asteroids by converging series, admitting of tabulation; by G. W. Coakley.

Analysis of a Bituminous Earth from Brazil; by E. N. Horsford.

Section B—Natural History and Geology.

Remarks upon certain points in Ichnology; by Edward Hitchcock.

Synchronism of Coal Beds in the Rhode Island and Western United States Coal Basins; by C. H. Hitchcock.

Geology of Newport and Vicinity; by C. H. Hitchcock.

Additional facts respecting the Clathropteris of East Hampton; by Edward Hitchcock.

Description of brecciated trachytic Dykes in Shelburne, Vt., with special reference to their temperature, when formed; by Edward Hitchcock.

Upon a Diatomaceous Earth from Nottingham, Calvert Co., Md.; by C. Johnson of Baltimore.

Jottings on the Geology of the Eastern part of Maine, &c.; by W. B. Rogers.

On the recent discovery by Mr. Norman Eastop, of Fossils in the conglomerate of Taunton River; by W. B. Rogers.

On the origin and stratigraphical relations of the Trappean rocks of Lake Superior; by J. W. Foster and J. D. Whitney.

On the Lead Region of the Upper Mississippi; by J. D. Whitney.

On the arrangement of the Museum of Comparative Zoology at Cambridge; by L. Agassiz.

On the origin and distribution of the Sediments composing the stratified rocks of North America; by J. S. Newberry.

On the Surface Geology of Western America; by J. S. Newberry.

Some points in the Surface Geology of the Northwest; by J. D. Whitney.

Remarks on the distribution of Gold in Veins, with notes upon remarkable Gold Specimens from Georgia; by Wm. P. Blake.

On the Ethnological Value of the imitative faculty in relation to the characteristics of ancient and modern American Races; by Daniel Wilson.

On the Petroleum Wells of the Mississippi Valley; by J. S. Newberry.

Description of a new species of Trilobite of the Genus *Conocephalites*, from the Potsdam Sandstone; by F. H. Bradley, with notes by E. Billings. Presented by B. Silliman, jr.

On Certain Phenomena of the Great Dismal Swamp in Virginia and North Carolina; by Nathan B. Webster.

On methods in Zoology; by Louis Agassiz.

On the American Reindeer; by L. E. Chittenden.

On the food of Birds, with special results in the investigation of the food of the Robin, (*Turdus migratorius*); by J. W. P. Jenks.

On the later extinct Floras of North America; by J. S. Newberry.

Age of the so-called Taconic Rocks in Vermont; by C. H. Hitchcock.

On the origin of the Prairies of the Northwest; by J. D. Whitney.

In obedience to a pledge given in 1859, that the next meeting should be held in some Southern city, the Association adjourned to meet in Nashville, Tennessee, in April, 1861, the exact date to be fixed by the local Committee.

The Officers of the Nashville Meeting are, President, F. A. P. Barnard, LL.D., President of the University of Mississippi; Vice President, Dr. Robt. W. Gibbes, of South Carolina; General Secretary, Prof. J. W. Mallet, of Mississippi; Secretary, Dr. A. L. Elwyn, of Philadelphia.

2. *Reclamation by Gov. Stevens of Washington Territory.*—Prof. B. SILLIMAN, Jr.—*Sir* :—I send you herewith a copy of my final Report and Narrative of the Exploration of the Northern route for a Pacific Railroad, and desire respectfully to ask you to examine it in connection with a volume entitled Natural History of Washington Territory, by Drs. Cooper and Suckley, also sent with this.

It is due to one of the authors (Dr. Suckley) that I should here remark that when he was informed of the injustice done me, he frankly admitted that I had a right to be dissatisfied and that nearly all the facts complained of occurred during his absence in California—that when he returned he found the title pages and the chapter on Meteorology had been printed under the direction of Dr. Cooper, my friend and neighbor, and that he supposed, from Dr. Cooper's relationship to me, that my permission had been given to the incorporation of the chapter on Meteorology. He also added that as an *amende honorable* he should send notices over his signature to every holder of the book and also one to be published in your Journal, stating that my name was inadvertently left out of the title page and that I was the *sole* author of the chapter on Meteorology. Therefore wherever Dr. Suckley's name is referred to, it is simply because he appears as joint author of the "Natural History," and not because I attach any blame to him in regard to the matter.

An examination of my report will show you that this "Natural History" is a portion of my own Report. I will first call your attention to their letter addressed to me transmitting their volume. You will find it at the close of the volume (Appendix C.). In that letter, as will appear from its contents, they transmit the portion of my Report immediately following the alphabetical Index and preceding the appendices.

If you will examine the "Natural History" you will find that it contains Chapter IV. of my Report on Meteorology, copied page for page; that this chapter is illustrated by several views taken from the body of my Report, and that my isothermal chart is to be found at the end of the volume. By turning to my own Report, it will be observed that this chapter on Meteorology is an integral portion of my own personal and official Report of the route, follows the geographical memoir and precedes the estimate of the cost of a railroad on the northern route.

You will also observe upon page viii. of the preface to the "Natural History" that the authors (Drs. Cooper and Suckley) state that none of the plates illustrating their volume have been before published in any of the series. If you will compare these pictorial views with those in my final Report and narrative, you will find them to be identically the same, having been struck off from the same stone, by the same person, and therefore cannot be new.

My object in addressing you is to expose the plagiarism of Dr. Cooper and to show his injustice to myself.

It being doubtful whether extra copies would be ordered by Congress, I gave my consent to the Government printer's striking off some extra copies of the Natural History Report for the use of Drs. Cooper and Suckley, they bearing the expense, and the pretended edition of Baillière Brothers was struck off in this manner. I did not give my consent to the incorporation of the chapter on Meteorology, or to the use of the

plates, or to the use of the isothermal chart. It was done without authority, without the least consultation with me, nor was I aware till I saw the bound volume that they had any intention of the kind.

Now turn to their title page. 1. My name is altogether omitted from it, whereas since the work is simply a portion of my own report, my name should have been there. 2. The title states that Drs. Cooper and Suckley are the authors of the work, and of course the authors of Chapter IV. The following extract from the title makes this claim:

"Being those parts of the final Reports on the Survey of the Northern Pacific Railroad route, containing the Climate and Physical Geography, with full catalogues and descriptions, &c., by J. G. Cooper, M. D., and Dr. G. Suckley, U. S. A., Naturalists to the Expedition." 3. The title shows new matter added, which, under the arrangement referred to, was certainly inadmissable.

I will now call your attention to Baillière's advertisement which I send you with the "Natural History." It is there set forth that Chapter IV, on Meteorology, is by Gov. Stevens and Dr. Cooper.

By reference to the date of my letter to the Secretary of War, transmitting my Report, and the date of Drs. Cooper and Suckley's preface, it will appear that on the 7th of February, 1859, this chapter was sent in to the Secretary as my exclusive work and was so published by the Senate. In November following it is claimed by Drs. Cooper and Suckley in their title page as their work, and in Baillière's notice as the joint work of Gov. Stevens and Dr. Cooper. The priority of the Senate publication establishes the plagiarism.

The authorship of the chapter in question is exclusively my own. Dr. Cooper has no pretext to any portion of the authorship. He was employed by me under the authority of the Department, at the rate of $125 per month, to assist me in my Report. He did assist me on this chapter and in other portions of my Report. I prescribed the mode of investigation, supervised the work daily and prepared the Report. The mode of investigation, reasoning, deductions and conclusions are my own and not Dr. Cooper's. My attention had long been given to the subject. The Doctor was patient under my direction in preparing tables and collecting data. It is not only a most extraordinary case for an employé to assume the authorship of an article when he simply did the work of an employé, but it has been long since established judicially that all such claims are utterly untenable.

Why did not Dr. Cooper claim the authorship when I sent in the Report to the Secretary? Why did he not claim it when he was employed by me in assisting me in the tables to be found in it? He was during this whole time and for many months subsequently at my house every day. Why did he not claim the authorship when the Senate Report was published? It was published before the date (November) of their preface.

Why has it been done clandestinely, my first notice of it being seven months after the Senate publication?

I had a right to expect better treatment at the hands of Dr. Cooper than this, since at the time he was contemplating claiming authorship to an article of which he knew I was the author, he was pretending friend-

ship towards myself, and afterwards did seek and obtain through my influence a position with a Government expedition.

I have respectfully to request that you will publish this communication in your valuable Journal. I am, Sir, very respectfully,

ISAAC I. STEVENS.

3. *Dr. Suckley's Disclaimer.*—Editors Silliman's Journal:—Gentlemen:—You will confer a favor by stating in the next number of your valuable Journal that the authorship of the Chapter on Meteorology in Cooper and Suckley's work on the Natural History of Washington Territory should be accorded to the Hon. Isaac I. Stevens, whose name was unfortunately omitted from the title-page. Notices to this effect have been sent to all the owners of copies.

Very respectfully, your ob't servant, GEORGE SUCKLEY.

New York, July 31, 1860.

4. *Stereoscopic Advertisements;* by ELI W. BLAKE, JR.—Prof. H. W. Dove, to whom we are indebted for so many beautiful stereoscopic experiments, in his *Optische Studien*,* gives a specimen of stereoscopic printing to illustrate the double refraction of Iceland Spar, as seen in *binocular vision.*

This effect is produced by printing for the left eye, lines in the ordinary manner, while for the right eye, the alternate lines are slightly advanced. Upon combining the two by means of the stereoscope, the printing appears to be in two planes, more or less distant from each other.

Should the "spacing" of the two not correspond *exactly*, single words will rise towards or fall back from the eye; thus by varying the spaces between the words even in the slightest degree, a marked effect is produced. In this age of advertising it might be worth the while of enterprising individuals to print their advertisements in a stereoscopic form. It would certainly gain for them a more general and careful study than this class of literature can generally command. I annex, as an example of this mode of printing, a stereoscopic advertisement of this Journal, which may be observed by simply placing the instrument over it.

5. *Paraselena and Lunar Rainbow;* by Lieut. J. M. GILLISS.—When returning from Washington Territory and at 9 P.M. of July 26th, we were witnesses to atmospheric phenomena so rarely seen together that a brief notice is offered for record in the Journal.

The steamer's position was lat. 44° N.; long. 124° 30′ W., and, consequently, some 20 miles N.W. of Umpqua river, in California. The stars shone brightly overhead and the atmosphere was perfectly calm, but loaded with a wet fog or mist. At the time specified, the moon was 8¼ days old and its altitude about 20°. Its disc was enveloped in bright mist, which extended in a well-defined circle of 30′ to 32′ diameter. This mist-ring was surrounded by an exquisitely marked halo of about 3° diameter, whose colors were blue, pearl-white and reddish-pink, the blue color nearest to the moon. The halo lasted from 15^m to 20^m, its colors gradually fading, until they were no longer distinguishable, though the bright inner mist-ring continued much longer. When it had disappeared stars were visible to within 15° of the horizon.

* Optische Studien von H. W. Dove. Berlin, 1859.

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Prior to the formation of the halo, and for some time after, we ceased to see it, there was a perfectly formed fog or mist bow to the north of the ship. At times, a slight tinge of dirty red could be traced on its outer border and near the sea, though the predominant color was a murky white.

At $9^h\ 30^m$ a faint secondary bow was formed of 3° lesser radius, but it continued only a few minutes.

6. *Oil Wells of Pennsylvania and Ohio.*—A strong impulse has been given to the explorations for petroleum by the success of the well at Titusville, Pennsylvania, worked for some years past by the Pennsylvania Rock Oil Company, on Oil Creek. In a circuit of five miles from Titusville, there were early in July, over 400 wells in progress, and 100 at work, raising from ten to fifty barrels daily from depths varying from 40 to 300 feet. The crude oil is dichromatic, having by transmitted light a dark brown color, which by reflected light is greenish or blueish. Its density is 0·882. It is even in warm weather rather thick, and in cold weather is more stiff, but even at −15° is still fluid. Its odor is strong and peculiar from the Pennsylvania wells—but from the wells at Mecca, Ohio, it is nearly odorless. It boils at a very high temperature, but begins to distill a thin colorless oil, even at 212° F. By fractional distillation, I obtained from 304 grams of crude oil of Titusville:—

	Temperature.		Quantity.	Density.
1st Product	at 100°C. = 212°F.	(acid water,)	5 Gms.	
2d "	" 140°C. to 150°C.=284° to 302°F.		26 "	·733
3d "	" 150°C. to 160°C.=302° to 320°F.		29 "	·752
4th "	" 160°C. to 170°C.=320° to 338°F.		38 "	·766
5th "	" 170°C. to 180°C.=338° to 356°F.		17 "	·776
6th "	" 180°C. to 200°C.=356° to 392°F.		16 "	·800
7th "	" 200°C. to 220°C.=392° to 428°F.		17 "	·848
8th "	" 220°C. to 270°C.=428° to 518°F.		12 "	·854

The *boiling points* of these several fluids present some anomalies, but are usually progressive, thus,

No. 2.	common boiling at 115°C.=239°F.	remained constant at	228°C.=442°F.
3.	" " 120°C.=248°F.	" "	270°C.=518°F.
4.	" " 140°C.=284°F.	" "	290°C.=554°F.
5.	" " 160°C.=320°F.	and still rising at	308°C.=581°F.
6.	" " 135° C.=275°F.	rose rapidly above the range of mercurial thermometer.	
7.	" " 135°C.=275°F.	and still rising at	305°C.=305°F.

The first five products remained entirely fluid at the low temperature of −15° F.

At the town of Mecca, Trumbull County, Ohio, many Oil Wells have lately been bored. We learn from Dr. J. S. Newberry, who has lately visited them, that only two were as yet considerably productive. One at 'Powers Corners,' which yields three or four barrels of crude oil, daily,—and the second has rewarded the industry of two poor Germans by the unexpected yield of twenty-five gallons hourly, or from twelve to sixteen barrels daily. Over fifty wells were already in progress. Many of them yielding oil from the sand near the surface, and all easily bored—often for a well to the depth of fifty feet at a cost not above fifty dollars—without the pumping apparatus and lining. Oil springs have been known in both

these districts for over fifty years, and were regarded by the agricultural population with disgust, as spoiling their water—until they found them an unexpected source of wealth. Both these districts are within the coal region—but the Pennsylvania locality, at least, is below the coal-bearing rocks.—s.

7. *Artesian Well at Columbus, Ohio,—rate of increase in temperature at 2,575 feet depth.*—In a letter to the Editors from T. E. WORMLEY, Esq. dated Columbus, Ohio, August 6th, 1860. Dear Sirs:—I herewith send you the temperature of the artesian well in our city, at a depth of 2,575 feet.

A few days since a Walferdin's Thermometer, placed in a glass tube filled with water, and this enclosed in a strong iron case, also filled with water, was lowered to a depth of 2,475 feet, where it remained for twenty-five hours, it was then sunk to the bottom of the well, a depth of 2,575 feet, where it remained for forty minutes. Upon the withdrawal of the instruments, it was found to have registered 88° F. Assuming this to be the temperature at the bottom of the well, and also assuming as correct data, that the temperature is uniformly 53° F. at a depth of 90 feet, we have an increase of 1° F. for every seventy-one feet.

8. *Salt Wells in Michigan.*—At *Saginaw*, Michigan, a well 669 feet deep has been sunk, passing through the coal measures and reaching about to the top of the Devonian. Its water is half saturated with salt and has the temperature at surface of 54° F.

At *Grand Rapids*, Kent County, Michigan, on the Grand River, the salt well is 661 feet deep—but as it begins 445 feet lower down in the section than the Saginaw well, the whole section presented by the two is over 1,100 feet. Careful sections of all the beds passed through have been preserved by Mr. George A. Lathrop, our informant, who will communicate them to the State Geologist.

It would be interesting to record the bottom temperature of all artesian wells, by a Walferdin's thermometer, and we trust our correspondents will take care to send us such results.

BOOK NOTICES—

1. *A Treatise on Elementary and Higher Algebra;* by THEODORE STRONG, LL. D., Prof. of Math. and Nat. Philosophy in Rutgers College, etc. New York. Pratt, Oakley & Co. 1859.—This work does not belong to the class of elementary compilations with which our schools and colleges are so abundantly supplied, but is a fresh and original treatise, stamped on every page with the peculiar impress of the author's mind. It contains moreover several positive additions to the Science while many of the demonstrations of well known propositions are new and highly suggestive. The leading idea of the author appears to have been to set forth the theory of the resolution of numerical equations in a new manner; and accordingly this subject is entered upon at as early a stage of the book as possible. Thus he begins its discussion under the head of *Multiplication*, continues it successively under the heads of *Division*, *Geometrical Progressions*, and *Undetermined Coefficients*, and finally completes it in two chapters, near the close of the volume, on the "Limits of the Real Roots of an Equation of the *n*th Degree,"—and the "Development of the Real and Imaginary Roots of an Equation of the *n*th Degree." This distribution of the subject, however, has not conduced to clearness, while it has given the work a character which renders the word

"Elementary," introduced into the title, quite inappropriate. We should not have thought it necessary to make this remark had not the author in his preface declared that the work was intended "to give a full view of the present state of the science," and also to be adapted "to the wants of teachers and pupils not only in our Colleges and Academies, but *in schools of a primary character*." Dr. Strong has risen to such heights in the science and has so long been an industrious explorer on its confines, that he has lost sight of the paths by which the novice must necessarily be introduced. But assuming the reader to have acquired a fair knowledge of algebra, including the general theory of equations, we would recommend him to read this work as a *review* in which the science is rearranged in a special order, and a new light is thereby thrown upon many of its processes. For Dr. Strong is a master of his subject and handles the most difficult parts of it with something of that kind of giant ease which we feel and admire so much in the great Euler.

From this statement, the reader will readily understand that a minute review of this work, pointing out all that is peculiar in methods, or in arrangement of methods, would be a profitless and superfluous labor. We will make but a single remark upon that part of the book which is prominently set forth as new and important both by the author and his reviewers—namely: the Solution of Cubic Equations "by pure algebra." The author's solution is certainly new, (at least new to us,) and extremely ingenious; but we can hardly concede that it is the only solution "by pure algebra," as appears to be claimed. For if the term "pure algebra" is used to distinguish the solution from that which is obtained by trigonometry, then we have other methods of solving the "irreducible case of Cardan's rule" by "pure algebra," quite as complete on this score as that of Dr. Strong. He obtains the roots only by a series of successive approximations, and does not present them in the form of a finite algebraic formula. Horner's method of solving equations appears to us to meet the case as well as Dr. Strong's, both being based upon successive approximation, and in practice Horner's method will generally be far more expeditious. Again, the roots may be developed in converging series (as was done by Nicole as long ago as 1738) and thus obtained by "pure algebra." As compared with the method by series, Dr. Strong's solution possesses the decided advantage that it *always* approximates with tolerable rapidity, the rate of approximation being nearly the same in every case; while the series of Nicole converge with more or less rapidity according to the relation existing between the coefficients of the given equation, and in certain cases the convergence is so slow as to render the series *practically* useless. For example, the terms of the series which express the roots of the equation $x^3 - 23x = 30$, will depend upon the successive powers of the fraction $\frac{24389}{24389}$, which differs so little from units, that one thousand terms of the series would barely suffice to determine the first decimal place of the root, whereas some ten or twelve approximations according to Dr. Strong's method will determine the roots accurately to six decimal places. This method, therefore, may be justly claimed as an addition to the science, as well as the solution of Binomial Equations, the new demonstration of the Binomial Theorem, the development of the roots of equations, series, etc., all of which will be read with pleasure by advanced students.

2. *Contributions to the Palæontology of Iowa, being descriptions of new species of Crinoidea and other fossils,*—(supplement to vol. i, part ii, of the Geological Repository of Iowa); by JAMES HALL.—Eight sheets of an early copy of these contributions have reached us from the Author in advance of publication. As we hope soon to receive a complete copy, we reserve a list of its contents for a second notice.

PERSONAL.—Prof. DANA returned to the United States early in August, improved in health by his European visit, but not yet in a state to permit the resumption of his accustomed labors. LADY FRANKLIN was passenger on the same ship.

Prof. C. U. SHEPARD, and Prof. ALEXIS CASWELL of Brown University are among the American tourists in Europe this season.

Prof. ELIAS LOOMIS, LL.D., late of New York University, has been elected to the chair of Natural Philosophy and Astronomy in Yale College made vacant by the death of Prof. D. Olmsted. Prof. Loomis is now in Europe for the purpose of making additions to the physical cabinet of Yale College.

JOSEPH E. SHEFFIELD, Esq., a citizen of New Haven, distinguished for his enlightened liberality, has at his sole charge, prepared a large and commodious building for the uses of the YALE SCIENTIFIC SCHOOL, embracing ample apartments for the Engineering, Mechanical and Physical departments—a perfectly appointed chemical Laboratory fitted for thirty special students, and private laboratories for the Professors—a Metallurgical Laboratory—Lecture rooms, both private and public—and two museum rooms—forming altogether a scientific Establishment, second to none in this country. It will be ready for occupation at the opening of the fall term.

TRANSACTIONS OF THE AM. PHILOS. SOCIETY, Phila., vol. xi, new series. Part III, 1860.—Art. xvi, p. 187–258, Revision of Buprestidæ of the U. S., with a plate. By *John L. LeConte, M.D.*—xvii, p. 259–402, Analytic Orthography; an Investigation of the sources of the voice and their Alphabetic notation. By *Prof. S. S. Haldeman.*

PROCEEDINGS OF AM. PHIL. SOC. PHILAD., 1860, January to June.—p. 174, Deaths of members announced—p. 175, Phosphoresence of the Diamond; *Dr. Emerson.*—p. 176, Gale of July 9th and 10th, 1860—Effects upon Philadelphia Gas Works; *Prof. Cresson.*—p. 177, Sanscrit and English analogues; *Pliny E. Chase.*—Geology of the Arctic Archipelago drawn from Mc Klintock's narrative, *J. P. Lesley.*—p. 295, Registering Thermometer; *James Lewis.*—p. 297. Biographical notice of the late THOMAS NUTTALL—p. 320. Optical education: *Dr. Emerson.*—Obituary notice of JOSEPH ADDISON ALEXANDER; *John Leyburn, D.D.*

TRANSACTIONS OF THE ACADEMY OF SAINT LOUIS, (Mo.;) vol. i.—p. 583, Observations on the cretaceous strata of Texas; *B. F. Shumard.*—p. 606, Descriptions of new cretaceous fossils from Texas; *B. F. Shumard.* The author describes the following: *Nautilus Texanus; Janira Wrightii; Ostrea quadriplicata; O. bellaplicata; Cidaris hemigranosus*—p. 624, Description of five new species of Gasteropoda, from the Coal measures, and a Brachiopod from the Potsdam Sandstone of Texas, *B. F. Shumard,* viz: *Pleurotomaria Brazoensis; P. tenuistriata; P. Riddellii; P. glandula; Murchisona Texana; Orthis Coloradoensis.*

MÉMOIRES DE L' ACADÉMIE IMPÉRIALE DES SCIENCES, ARTS ET BELLES-LETTRES DE DIJON, second series, Tome vii, 1858–1859. Dijon, 1859, 800 pp., scientific contents *catalogues des Insectes coléoptéres du département de la Cote-d'or (suite), par M. Rouget.*

Une visite á la grotte de Fouvent (Haute-Saône): Ossements fossiles et debris de l'industrie humain; memoire posthume de *M. Nodot.*

POSTSCRIPT, September 7.

LeVerrier's Report on the Solar Eclipse of July 18, 1860, *at Tarazona in Spain*, (*L'Institut*, Nos. 1387–88–89. Aug. 1–16.)—[At the last moment, and after our notices of this phenomenon were printed (see pp. 281, 285, 288), we have received LeVerrier's Report of the Observations of the French Expedition to Spain, made to the Minister of Public Instruction, which we hasten to lay before our readers, slightly condensed, although other matters which some of our correspondents will naturally look for here are thereby displaced. The interest with which LeVerrier's new views of the physical constitution of the Sun will be read is our apology to all such.]

LeVerrier was accompanied to Spain by Messrs. Yvon Villarceau, and Chacornac, who were occupied chiefly with determining the height and position of two or more of the luminous appendages. M. Foucault studied the corona, and made the photometric and photographic experiments. M. LeVerrier observed the astronomical phases of the phenomenon, and was also charged with the duty of obtaining an exact description of the whole scene. Two telescopes on Foucault's plan were devoted to the measurements, being provided with micrometers of peculiar construction, devised by Yvon Villarceau for rapid and easy manipulations in the dark. Two excellent telescopes of 6 inch aperture (one for the use of the Spanish observers) were also provided, to which must be added the photographic apparatus, a meridian circle, chronometers, barometers, seekers, and lastly the great meridian instrument belonging to the War Department, and with which the longitude was determined—forming a grand total of scientific baggage which on the 28th of June was dispatched for Spain. The outfit of the English expedition was even yet more considerable. Early in July Mr. Yvon Villarceau joined the instruments at Tudela in the center of Spain, on the banks of the Ebro, and immediately proceeded with them to Tarazona and to the chosen station called the *Sanctuaire*, 1,400 metres (=4592 feet) above the sea. M. LeVerrier and Foucault, fearing clouds, descended on the morning of the 18th to a plateau near the cemetery of Tarazona where the weather was magnificent during the whole eclipse. Passing the description of the contacts and observations for time, &c., we note that at totality they found the general illumination of the atmosphere much greater than the relation of former observers of total eclipses had led them to expect, so that they could read and write easily without using their lamps.

Says LeV.: "The first object which I saw in the field of the telescope after the commencement of totality was an isolated cloud separated from the moon's border by a space equal to its own breadth, the whole about a minute and a half high by double that length. Its color was a beautiful rose mixed with shades of violet, and its transparency seemed to increase even to brilliant white in some parts. A little below on the right two clouds lay superimposed on each other, the smaller above, and the two of very unequal brilliancy. The rest of the western edge of the disc and the lower part showed nothing more than the corona, the light of which was perfectly white and of the greatest brilliancy. But 30° below the horizontal diameter on the east I discovered two lofty and adjoining

peaks, the upper sides both tinted with rosy and violet light while the lower sides were brilliant white. I do not doubt that the toothed form I assign to these peaks is real, which as it contrasted with that of the first appendages I have described, I verified with great care; moreover, in shifting the telescope, whose high power permitted a sight of only a small part of the solar disc at one time, I saw a third peak a little higher, also tooth-formed, and resembling the two others in color and form, differing only in its larger dimensions. The remainder of the disc offered nothing remarkable, and on returning to the upper region I found the two first described clouds unchanged. As the moment of reappearance of the Sun approached, and while waiting for the first rays, I made, during about 20ˢ, perhaps my most important observation. The margin of the disc which two minutes before was entirely white was now tinged by a delicate fillet of unappreciable thickness of a purplish red—then as the seconds glided by, this fillet enlarged by degrees and formed soon around the black disc of the moon, over a breadth of about 30° a red border perfectly defined in thickness, crescent-formed and with an irregular outline above. At the same instant the brilliancy of the part of the corona which during the last second emerged from behind the moon's disc was exalted so rapidly that I was in doubt if the sun's light was not returned. It was only on the reappearance of the direct rays, the brightness of which obliterated in turn the corona, that I was sure of the nature of three phenomena present at the same time, which I thus sum up.

1. The visible part of the emergent Sun over its whole breadth and up to the height of seven or eight seconds was covered by a bed of rosy clouds, which appeared to gain in thickness as they emerged from behind the disc of the moon. Must we believe that the entire surface of the sun is overspread at a small elevation as it is strewn with faculæ, and that the roseate clouds are emanations, appearing like spots on the sun's disc?

2. The intensity of the light in the corona which is always white, varies with great rapidity in the immediate vicinity of the Sun's disc.

3. The reappearance of the direct sunlight was at $3^h\ 0^m\ 49^s{\cdot}0$. Total obscuration continued $3^m\ 14^s{\cdot}3$. The disc of the moon completely cleared the Sun at $4^h\ 6^m\ 20^s$."

[M. Foucault's interesting observations on the photographs, etc., are unavoidably postponed for want of room.]

LeVerrier goes on to state that the observation of his party authorize, in his opinion, important modifications in the generally received notions respecting the physical constitution of the Sun. Arago in his notice of solar eclipses—says, "where exist the reddish flames with well defined outlines which during the total eclipse of the 8th of July, 1842, passed considerably beyond the outlines of the lunar disc? These flames were either in the Moon, or in the Sun, or in our atmosphere; unless, indeed, denying their actual existence, we regard them as an effect of light, for example as phenomena of diffraction."

The two last suppositions have found few partisans. Before adopting any hypothesis it is necessary to decide by observation a certain feature of the phenomenon. During the eclipse, the disc of the moon moves across the disc of the Sun. But do these reddish clouds follow the moon in

its movement? or does each cloud remain invariably above the same point on the solar disc? In the first case the origin of the luminous clouds is to be sought in the Moon; in the second case, these clouds belong to the Sun. For clearness sake, assume the latter supposition, and observe what appearances should present themselves when the lunar disc passes like a screen over the whole. Consider first a cloud situated on the east and adherent to the Sun's limb. This object will be visible at the instant when totality commences. The advancing moon will regularly, at the rate of a half second of arc in a second of time, cover with its limb successively the lower then the middle and lastly the higher portions—thus constantly diminishing the height of the cloud. For a cloud situated on the west, these appearances will be reversed, its magnitude increasing as the moon gradually uncovers it. If then the roseate appendages seen during a total eclipse, depend on the sun, the fact should appear by the variation in height between those which appear in the east and the west. The phenomena will appear otherwise if the clouds appertain to the moon.

In the absence of equatorial solar clouds, the question in dispute can still be decided by observations on those seen on the south or north of the disc. The height of these clouds ought not to vary, it is true, whether they belong to the moon, or to the sun, but in the latter case, carried away by the sun they would be displaced on the lunar disc with a certain velocity, while if they are adherent to the moon's disc, they would not be so displaced Hence the study of the height of the luminous clouds, whether east and west or north and south, has the highest interest. All the elements of the desired demonstration are found in the Spanish observations. In my first report, I mentioned the successive increase in thickness of a band of rosy clouds visible from the east to the end of the eclipse. Messrs. Yvon Villarceau and Chacornac have carefully noted the motions of a cloud, situated on the north. This cloud, according to M. Villarceau, in two minutes' time was displaced $3\frac{1}{2}°$ on the moon's disc, in moving to the west. The measurements of M. Chacornac cover an interval of over six minutes in time, in which the cloud moved $11\frac{1}{2}°$.

Beautiful observation, and one which could not have been hoped for! We see that the duration of the motion studied by Chacornac, much exceeds the time of total obscuration. The last measure was made more than three minutes after sun-light had reappeared! It is important to note, among other points that this was not done with a cloud vaguely seen after the return of sunlight, but fortunately it was a measurement so carefully made as to be a guaranty against the possibility of illusion. It should be added that the displacement of the luminous cloud determined by the observations made at the *Sanctuaire*, is precisely equal to that required by calculation, assuming the cloud to belong to the Sun. There remains, then, no foundation for a doubt, as to the nature of the rosy clouds which have been variously called flames, mountains, protuberances, and clouds.

The observation on one of the appendages, perfectly isolated from the disc of both the Sun and Moon and of a sharply pronounced character, and on the other the appearance of a rosy band on the west at the moment of emersion, and the rate of motion of a second appendage, fixed by Villarceau and Chacornac, prove that these objects belong to the Sun.

Let us then hereafter give the name of *solar clouds* to the rosy appendages which become visible when the solar light is sufficiently dimmed.

A few words more will finish the description of the phenomenon and of the observations. Ismaïl-Effendi, a young Egyptian attached to the Paris Observatory for three years past, a very expert astronomer, and who accompanied the French expedition to Spain, has sent me a drawing which proves the appearance of luminous clouds in the east immediately before the commencement of the eclipse. The clouds in question form a slightly elevated but continuous band embracing 90° of the outline of the Sun. This band was not long visible, but was eclipsed behind the lunar disc, and it had in effect ceased when I passed over this region in exploring the whole periphery with a power which allowed me to see only portions successively.

The magnetic observations were made at Paris, the variations being sensibly simultaneous for the whole of Europe, and M. Desains, who took note of the magnetic observations, detected no perturbations during the eclipse.

Physical constitution of the Sun.—A reconstruction or even a complete abandonment of the theory hitherto prevalent as to the physical constitution of the Sun appears to me essential. It must give place to one far more simple.

We have been hitherto assured that the Sun was composed of a central dark globe; that above this globe existed an immense atmosphere of sombre clouds, still higher was placed the photosphere, a self-luminous, gaseous envelope, and the source of the light and heat of the sun. Where the clouds of the photosphere are rent, says the old theory, the dark body of the Sun is seen in the spots which so frequently appear. To this complex constitution must be added a third envelope formed of the accumulation of roseate clouds.

Now, I fear that the greater part of these envelopes are only fictions—that the Sun is a body, luminous, simply because of its high temperature, and covered by an unbroken layer of roseate matter whose existence is now proved. This luminary thus formed of a central nucleus, liquid or solid, and covered by an atmosphere, falls within the law common to the constitution of celestial bodies.

[M. LeVerrier goes on to discuss with some detail the solar spots in the light of these new views, but this we must defer for another occasion. It is certain that a subject of so much interest will command much consideration from physicists and astronomers and we shall take care to give it the attention it deserves.

Nor will the question be settled peaceably—already M. Faye (*Comptes Rend.*, Aug. 13) in presenting to the French Academy a long letter from Baron Feilitzsch with an account of his observations (also in Spain), declares it to be his opinion as well as that of Baron F. that the eclipse of 1860 furnishes the most decisive evidence in favor of the opinion which refers the corona and the luminous clouds to simple optical appearances, and not to the essential constitution of the Sun or of his atmosphere. M. Faye adds that the opinion appears to be confirmed by a comparison of the results of other observers—that the Sun has no atmosphere and that the appearances recorded are purely optical!—Eds.]